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THE

EDINBURGH
MEDICAL AND SURGICAL
JOURNAL:

EXHIBITING

A CONCISE VIEW

OF THE

LATEST AND MOST IMPORTANT DISCOVERIES

IN

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THE

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No. CLXVIII.

CONTENTS.

PART I.—ORIGINAL COMMUNICATIONS.

ART. I. Report of Cases, &c. brought before the Liverpool Pathological Society, Session, 1845–46, which were illustrated by Pathological Specimens, Drawings, Casts, &c. Witth the Secretary's Address, Page 1

II. Observations on the plan of Treatment of Cases of Placenta Prævia proposed by Dr J. Y. Simpson, Professor of Midwifery in the University of Edinburgh, with a Statistical Report of such cases as have occurred in the practice of JAMES RUSSELL, F. R. C. S. England, . . . 47

III. On the Period of Puberty in Hindu Women. By JOHN ROBERTON, Esq. Surgeon, . . 56

IV. On Fractures of the Cervix Femoris, with Cases. By ALEXANDER WATSON, M. D. . . . 64

V. On Malarial Fevers as observed in Canada from 1838 to 1846. II. Analysis of repeated attacks in the same individual. By THOMAS STRATTON, M. D. . . 74

VI. An Experimental Inquiry into the Functions of the Ophthalmic Ganglion; with an application of the inferences obtained to the physiology of the Ganglionic System in general. By C. RADCLYFFE HALL, M. D. . . . 84

VII. Statistics of the Royal Infirmaries of Edinburgh and Glasgow; prepared from the Managers' printed Reports, and other authorized Documents. By J. THOMSON, Esq. . 108

PART II.—CRITICAL ANALYSIS.

ART. I. Scrofula; its Nature, its Causes, its Prevalence, and the principles of Treatment. By BENJAMIN PHILLIPS, F. R. S. . 117

II. Medical Notes on China. By JOHN WILSON, M. D. . 158

III. A Memoir on Amputation of the Hip-Joint, with a successful Case. By WILLIAM SANDS COX, F. R. S., &c. . 195

IV. 1. Historical Details relative to the Military Force of Great Britain, from the earliest periods to the present time, By HENRY MARSHALL, Esq.

2. A Historical Sketch of Military Punishments, in as far as regards Non-Commissioned Officers and Private Soldiers. By HENRY MARSHALL, Esq. . . 202

V. A Practical Treatise on Inflammation, Ulceration, and Induration of the Neck of the Uterus; with remarks on the value of Leucorrhœa and Prolapsus Uteri as Symptoms of Uterine Disease. By James Henry Bennett, M. D. . Page 226
VI. Lectures illustrative of various Subjects on Pathology and Surgery. By Benjamin C. Brodie, Bart. F. R. S. . 232

PART III.—Medical Intelligence.

Spermatozoa of the Bear, . . . 233
On the size of the Red Blood Corpuscles of Birds, . ib.
On the Cerebral Organ of Speech, . . 234
New Researches on the Movements of the Heart, . ib.
On the use of the Cerebro-Spinal Fluid, . . 235
New Researches on the Production of Fat, . . ib.
Case of Tubercle on the Tuber Annulare, . . 236
New Researches on the Œdema of new-born Infants, . ib.
Ulceration of the Gums in Children occurring in an Epidemic form, . 237
Dr Baly's Evidence on the Influence of Confinement in causing diseases, 238
Can Syphilis be communicated to the Lower Animals? . 248
On making Unfermented Bread, with observations, by a Physician, ib.
On the Treatment of Seminal Losses by Compression, . 250
Influence of the Quality of the Milk of the Nurse on the Health of the Child. 251
Successful case of the employment of Iodine in removing sparks of Iron from the Cornea, . . . ib.
Means of avoiding the great mortality following the great operations of Surgery, ib.
On Calculi of the Nasal Fossæ, . . . 252
Necrosis of the Maxillary Bones produced by the vapours of Phosphorus, 254
Case of Vagino-labial Hernia, . . ib.
Two cases of Prolapsus of the Uterus cured by excision of a portion of the vagina, 255
On the Treatment of Venereal Vegetations, . . ib.
Extract of Nux Vomica in Prolapsus Ani, . 256
Report on the Mortality of Edinburgh and Leith, . . 257

NOTICES TO CORRESPONDENTS.

Communications have been received from Dr Hall, Dr Watson, Mr Tait, and Mr Wilson.

The following works have been received:—

The Anatomy of the Human Body, by Jones Quain, M. D. Fifth edition, entirely new. By Mr Richard Quain and Dr Sharpey. Part II., p. lxxxv.–clxxxv., p. 425–678. An account of this work in our next.

Liebig's Physiology, applied in the Treatment of Functional Derangement and Organic Disease. With observations upon Hahnemann's Practice. Part I. The Heart, Lungs, Stomach, Glands, Joints, Bones, &c. By John Leeson, Practitioner of Medicine, M. R. C. S. England, &c. London, 1846. 8vo. Pp. 219.

Remarks on the Dysentery and Hepatitis of India. By E. A. Parker, M. B., late Assistant-Surgeon H. M. 84th Regiment. London, 1846. 8vo. Pp. 271.

Elements of the Theory and Practice of Medicine. Designed for the use of Students and Junior Practitioners. By George Gregory, M. D. Fellow of the Royal College of Physicians in London, Physician to the Small-Pox and Vaccination Hospital, &c. &c. Sixth Edition, with numerous additions and alterations. London, 1846. 8vo. Pp. 729.

Notes and Recollections of a Professional Life. By the late William Fergusson, M. D., Inspector-General of Military Hospitals. Edited by his son, James Fergusson. London 1846. 8vo. Pp. 248.

A Series of Essays on Inflammation and its varieties; tending to show that most Diseases either consist in Inflammation, or are consequences of it more or less remote. Essay I., the Natural History of the Disease; with Preliminary Observations. By Henry Clutterbuck, M. D., of the Royal College of Physicians, London. London, 1846. Pp. 67. 8vo.

On the Health of Towns as influenced by Defective Cleaning and Drainage. And on the Application of the Refuse of Towns to Agricultural purposes. Being a Lecture delivered at the Russell Institution, May 5, 1846. By William A. Guy, M. B. Cantab. Professor of Forensic Medicine, King's College. London, 1846. 8vo. Pp. 48.

A System of Surgery. By J. M. Chelius, Doctor in Medicine and Surgery, &c. &c. Translated from the German, with additional Notes and Observations. By John F. South, Professor of Surgery in the Royal College of Surgeons, England. London, 1846. 8vo. Pp. 193–320. Part XI.

On Disorders of the Cerebral Circulation; and on the connection between Affections of the Brain and Diseases of the Heart. By George Burrows, M. D., late Fellow of Caius College, Cambridge; Fellow of the

Royal College of Physicians, London, &c. London, 1846. 8vo. Pp. 220.

Introductory Lecture to a Course of Military Surgery, delivered in the University of Edinburgh. By Sir George Ballingall, Surgeon to the Queen, Regius Professor of Military Surgery in the University of Edinburgh. Edinburgh, May 1846. 8vo. Pp. 23.

The Economy of the Animal Kingdom, considered Anatomically, Physically, and Philosophically. By Emanuel Swedenborg. Translated from the Latin by the Rev. Augustus Clissold, M. A. Vol. I., p. 574. Vol. II. p. 426. London, 1845, 1846.

A Manual of Operative Surgery based on Normal and Pathological Anatomy. By J. F. Malgaigne, Professor, &c. Translated from the French by Frederick Brittan, A. B., M. D., M. R. C. S., London. Illustrated by Wood Engravings, from Designs by Dr Westmacott. London, 1846. 12mo. Pp. 586.

Meade's Manual for Students Preparing for Examination at Apothecaries' Hall, or other Medical Institutions. Second Edition, much improved, and corrected to the present time. London, 1846. 12mo. Pp. 536.

Remarks on Medical Reform. By Moses S. Buchanan, M. D., &c. Glasgow, 1846. 8vo. Pp. 28.

On Tubercles of the Brain in Children. By James Maxwell Adams, L. R. C. S. E., &c. Glasgow, 1846. 8vo. Pp. 32.

The Physiology of the Nerves of the Uterus and its Appendages. By Joseph Swan. London, 1846. 8vo. Pp. 31.

The Medico-Chirurgical Review and Journal of Practical Medicine. New Series. No vi., 88 of old. April 1846. London, 1846. 8vo.

The British and Foreign Medical Review, or Quarterly Journal of Practical Medicine and Surgery. Edited by John Forbes, M. D., F. R. S., &c. No. xlii. April, 1846. London. 8vo.

The American Journal of the Medical Sciences. Edited by Isaac Hays, M. D., &c. Philadelphia, 1846. April. No. xxii. New Series.

The London Medical Gazette. Session 1845–1846. No. 47. Friday, 20th March 1846.—No. 58, or 966. June. 5. No. 56, or 964 not received.

Weekly Tables of Mortality for the Metropolis. No. 12, Saturday, 21st March 1846, to No. 23, Saturday, 6th June 1846.

The Provincial Medical and Surgical Journal. Edited by Robert N. Streeter, M. D. No, 41, 42, &c.

The Northern Journal of Medicine. Edited by Dr Seller. No. xxiv.

Communications and Works for Review to be addressed to the Editor at Messrs Longman and Co., London; or Messrs Adam and Charles Black, Edinburgh.

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THE
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I. JULY 1846.

PART I.

ORIGINAL COMMUNICATIONS.

Art. I.—*Report of Cases, &c. brought before the Liverpool Pathological Society, Session* 1845–46, *which were illustrated by Pathological Specimens, Drawings, Casts, &c. With the Secretary's Address.*

Address by the Secretary, Thomas Inman, M.D., (London,) Senior Surgeon to the Liverpool Fever Hospital and Workhouse.

Mr President and Gentlemen,—Having at length arrived at the termination of another session, it becomes our duty to look back and see what progress we have made, and contemplate in its general bearings the matter which has come before us in an isolated form.

Of the diseases of the nervous system we have seen but few specimens, remarkable either for the extent of the injury or the rarity of the change. Two were especially interesting, in one of which the greater part of the *falx major* was ossified, while in the other a large lamina of bone was developed in the *dura mater.* The symptoms in neither instance were commensurate with the disease. Paralysis of long standing marked the latter; whereas the former was found in one who had never complained of any cerebral affection.

One instance only of disease of the brain has been exhibited, though many have doubtless occurred in the practice of our members. The reason for this must be sought in the way in which the examination of this organ is usually carried on, as well as the rapid decomposition which takes place after death. The former generally destroying the natural appearance of the disease while searching for its extent, the latter merging all parts, whether sound or unsound, into the same soft mass.

We have had, in addition to the two recorded last year, another instance of nervous tumour. It is to be regretted that these have not been hitherto examined by the microscope, with a view to ascertain their structure.

The development of a new structure in the optic nerve of a woman, aged 76, dead with melanosis of the eye of twelve years' standing, has been noticed. This, though of interest to those who take pleasure in minute anatomy, will not add any important information to the physiologist.

Two cases of injured spine have been exhibited; in both there was fracture arising from direct violence, causing a fatal termination in two or three days; in one there was continued priapism without any venereal excitement, in the other none whatever.

Three cases of gangrene of the lung have been commemorated. Notwithstanding the certainty said by some to attend the diagnosis of this disease, one only was recognised during life. In one, although the mischief was most extensive, the whole lung being in a state of diffuse gangrene, the fœtor of the breath though present was not so great as to attract general attention. No sooner, however, had death ensued, than the stench became insufferable. In another the disease was strictly symmetrical, being confined to a small spot on the surface of both lungs; no symptoms indicated its presence, these being either masked or prevented by the more severe complaint, puerperal peritonitis, of which the patient died.

We have only had one case of pneumonia before us, from which the patient would probably have recovered, had not the lungs been previously injured by cretaceous tubercles and emphysema.

Diseases of the heart have occupied the same prominent place on our table this year that they have previously done; five cases have been recorded, two of which were marked by sudden death.

One was especially interesting, in consequence of its extreme rarity, rupture of the left auricle being hitherto almost unknown. In this instance, though the death was sudden, there had been, for three days previously, symptoms of disease going on.

Another case of rupture has been recorded consequent upon softening and disorganization of a small spot in the inter-ventricular septum; had not the patient died of rupture of the aorta, it is probable a curious set of symptoms would have been produced.

Two were remarkable as being attended with hæmorrhagic diathesis, marked in the one case by bleeding from various internal organs, the nose, mouth, kidneys, and bowels, as well as by numerous spots of purpura on the skin ; in the other by simple purpurous patches about the abdomen and thighs.

Three cases of pericarditis have been detailed, which were accompanied by a number of valuable practical observations. One of these cases is deserving of especial attention, as it occurred in a powerful and robust young man, and was evidently produced by over-exertion in training for a boat race.

A case of rupture of the aorta near the heart has been mentioned, attended also with softening of the inter-ventricular septum, proving, of course, suddenly fatal. Another of aneurism of the aorta bursting into the lower part of the œsophagus, in which the death was instantaneous, no symptom except an occasional sensation of pain having previously indicated its existence.

An instance of aneurism of the cœliac axis, occurring in a young sailor, otherwise healthy, has been recorded, in which the progress of the disease was extremely rapid.

A singular case has been noticed in which, from caries of the posterior part of the sternum, a branch of the internal mammary artery was ulcerated. This had given rise to extensive effusion of blood, which made its exit between the false ribs, and diffused itself under the integuments over the whole front of the chest.

A very interesting and curious case has been presented to us, in which, from some supposed obstruction to the *vena cava descendens*, there was anasarca of the upper half of the body only. In consequence of treatment the dropsy gave way, when it was found that the venous circulation of those parts was being carried on in a great measure by the cutaneous veins of the trunk, communicating with those of the lower extremities. The patient has recovered sufficiently to leave the hospital.

Amongst the diseases of the digestive apparatus, we have had to record one case of ulceration of the œsophagus communicating with the trachea. The gullet was greatly enlarged at the part, though for a short distance only, and some discussion arose as to whether the case was one of abscess or cancerous degeneration.

An almost unique case has been detailed at length, in which a patient having died from hæmatemesis, was found to have suffered from rupture of one of the superior branches of the *coronaria ventriculi* where it anastomoses with the œsophageal. So small was the opening, and so free from any contiguous disease, that it was long searched for in vain, and at last discovered accidentally.

Six cases have been brought forward of that terrible and fatal disease—perforation of the stomach, and four others occurring in the practice of various members mentioned in debate. One only amongst them all occurred in a man. The proportion in which the patients had previously suffered from indigestion has been about one-half.

Various surmises have been made and many hypotheses formed respecting the nature and cause of this complaint, all of which have been successively abandoned. In one case only was there any peculiar appearance about the parts; there were three ulcers, at the bottom of two of which there was a distinct slough as if made with *potassa fusa,* and beginning to separate; in the third there was the same appearance to a more marked degree, with this exception, that the dead part had been torn through, leaving a ragged opening whose sides might be brought together and accurately fitted.

The variety of symptoms attending these formidable lesions has been small. In one, however, it will be recollected that though there was severe peritonitis, and the patient died in 36 hours, there was at one time an almost total absence from pain. Few of the patients have survived longer than two days, and no treatment has effected a cure. The use of solid opium in large doses seems to have been the most advantageous practice, and has in some tended to prolong life, though it has failed to cure.

Two cases have been mentioned of an extraordinary evolution of gas from the stomach, fatal in one case from rupture of the stomach and diffusion of the gas throughout the body, the symptoms being so unusual and unprecedented as to give rise to a suspicion of poisoning. In this instance the patient was a man, in the other the sufferer was a married lady. After an ordinary supper, of which her husband also had partaken, she retired to bed, was shortly afterwards heard moaning faintly, and found with her abdomen enormously distended, and almost dead from asphyxia; being suddenly raised she vomited an acescent mass, in which, so strong was the evolution of gas, that it resembled active boiling. Had not vomiting occurred it is probable that the termination of this case would have been similar to the former.

A remarkable malformation of the spleen will shortly be added to our preparations, the history connected with it being yet more strange. The patient, an adult man, appears to have enjoyed good health, and followed an occupation requiring great exertion. Some time previous to this fatal attack he had fractured his thigh and suffered from obstruction in the bowels. He again fractured his leg, and on being confined in the supine posture again suffered from obstruction, having at the same time a strong impression that the prone position would cure him. After death a supplemental spleen was found about the size of an orange, which had fallen into the pelvis and strangulated the gut by pressure.

We have had occasional opportunities during the past year of witnessing the ravages of ulceration and dysentery in the small intestines. Two cases only are of peculiar interest in which ulceration confined to the large intestine had appeared as a sequela of severe measles in young children, and proved rapidly fatal.

One case only of diseased liver has been exhibited of sufficient interest to attract attention. It occurred in a gentleman suffering from renal disease, and was of very rapid growth. The viscus was found enormously enlarged, weighing about ten pounds. It was found to be hard, homogeneous, of a pinkish-red colour; no traces of lobules or divisions of any kind could be seen; and the microscope proved that it was (except in parts) entirely free from fat, the cells being filled with solid matter which rendered them indistinct.

A case of cystose cancer of the testicle and lumbar region has been detailed, of a particularly interesting nature, from the similarity of the symptoms it produced to those of hernia being so great. An operation was resorted to, which for a time seemed to have given relief,—the patient, however, died three days afterwards.

A case of calculus of the prostate with fistulous communication between the bladder and rectum, in which two (calculi) had previously been removed from the bladder without any alleviation of suffering, has been recorded. After that in the prostate had been removed by operation, the patient immediately got well, the fistula entirely closing up.

Three or four cases of scrofulous depositions in the kidney have been detailed, in which during life there had been the greatest obscurity of symptoms. In one case, the patient, though passing daily in his urine what appeared to be pure purulent matter, was able to follow a business requiring both exertion and energy. In another no symptoms whatever indicated the presence of renal disease, and in the third none of the many physicians he had consulted had been able to detect either the seat or nature of his disease. Four cases were connected with phthisis, and one with scrofulous deposits elsewhere.

One case only of granular disease of the kidney has been presented to us, occurring in a patient suffering from hip-joint disease. As soon as the symptoms of the former were confirmed, those of the latter steadily declined, as if a true metastasis had occurred. In one case of diseased heart attended with purpura and hæmaturia, the kidneys presented a curious appearance in consequence of many of their urinary tubes being filled with blood. On microscopical examination the hæmorrhage was found to be confined to the urinary tubes, which were both distended and distorted, proving sufficiently that the hæmaturia had not been from the rupture of any single vessel.

We have had during the present session a medico-legal case of extreme interest brought under our notice, in which a family were poisoned by arsenic accidentally introduced into a well. It is to be regretted that the case did not meet with that full and searching investigation which such an important subject demanded.

An example of rupture of the uterus during labour has been recorded, in which that organ and the bladder were softened and almost disorganized. The case was further complicated by a strong fleshy band extending across the external orifice of the vagina.

Cases of corroding ulcer of the uterus, apoplexy of the placenta, and of ovarian dropsy have been brought forward. In the latter, the sac having been extirpated by Dr Clay, the patient recovered.

We have had an opportunity of examining microscopically a muscle of the thigh converted entirely into fat, from a patient who had been long paralytic. It had retained its usual size and shape, and the tendinous part was unaltered. No trace whatever remained of the striped fibres, their place being taken by a number of oil globules arranged in symmetrical rows. The specific gravity of the mass was 996.

1. *Polypous Tumour displacing the Globe of the Eye.* By R. Hibbert Taylor, M. D.

John Bent, a stout healthy-looking tailor, came to the dispensary for diseases of the eye, on account of dimness of sight, and protrusion of the right eyeball from the orbit of three or four months' standing. Two years previously he had experienced several attacks of epistaxis, the first of which was very profuse. A month or two afterwards he observed that the right nostril was becoming obstructed, and in the course of a year he was unable to breathe through it. Eight months before I saw him a large polypus had been extracted from the nostril in several fragments, and since then the passage had continued clear. A month after this operation he received an accidental blow with the end of a ladder upon the inner canthus of the right eye, which knocked him down. Immediately after a swelling began to form in this situation, attended with throbbing pain. It was opened with a lancet, and a considerable quantity of bloody purulent matter evacuated. The discharge continued during two months, gradually diminishing in quantity and consistence, and ultimately the wound closed up. While the discharge continued he had very little uneasiness, but when the wound healed he became subject to pain over the right eye-brow and temple, and the tumour at the inner canthus began to enlarge. During a month past he thought that there had been very little change in the symptoms.

On examination I found that the right eyeball was slightly protruded from the orbit in the direction downwards and outward. The upper eyelid and the space intervening between it and the superciliary ridge was swollen, and had a puffy œdematous feeling. The situation of the lacrymal sāc was occupied by a tumour, in size and form resembling a small olive, placed diagonally, extending from the superior and internal angle of the orbit downwards and outward towards its inferior margin. It was elevated so as to be nearly on a level with the bridge of the nose. When touched it had a dense elastic feeling, like a membranous bag tightly expanded with fluid, and it was somewhat tender on pressure. Traced with the finger it seemed to ascend close up to the superior and internal angle of the orbit, and in this situation its surface felt firm and hard, as if invested with bone. The integuments could be slightly moved over it, but the tumour itself seemed to be fixed.

The movements of the right eye-ball were natural, and harmonised with those of the other. The pupils were of medium size, and regular; the right iris had no independent motion, but moved freely when the left eye was open. With the right eye the patient distinguished light and shadow, and on looking at a printed page said that he saw something black; vision with the left eye was good.

The right nostril appeared to be larger than the left, and easily admitted the little finger to a considerable depth. He had no difficulty in breathing through either nostril, but his voice was somewhat deficient in sonorousness, as if the nostrils were stuffed.

My first impression on handling this tumour, and before becoming acquainted with the previous history of the case, was that it consisted of the lacrymal sac greatly distended with fluid. On making firm pressure upon it nothing escaped from the puncta or into the nostril, but this I had frequently observed to be the case in chronic drocyocystitis. The considerable size of the tumour was, however, rather unfavourable to the above supposition, as to its nature. To solve this point I punctured the tumour with a straight cataract needle, and afterwards with a narrow-bladed knife, to the depth of an inch, going quite through the swelling. Blood alone flowed, and that apparently from the integuments; the tumour remained as tense and elastic as before.

The history of the symptoms suggested another view of the case, viz. that it was a polypus protruding into the orbit, either from the nares or the frontal sinus. The displacement of the eyeball downwards and outward seemed to indicate that the progress of the tumour had been from the upper and inner angle of the orbit, to which point it could be apparently traced at its superior extremity. On turning to Dr Mackenzie's Treatise on Diseases of the Eye, I found a case quoted from Langenbeck very similar in appearances to the above, which was discovered to be dependent upon a polypous growth from the frontal sinus, which had made its way into the orbit. (Pract. Treatise, 3d edit. p. 63.)

With the view of obtaining some additional light upon the subject, I presented the patient at a meeting of the Medical Society, where he was carefully examined by several of the members. One considered the tumour to be of a steatomatous character, and unconnected with the polypus in the nostril. Another coincided with me in thinking that there was some connection between them, and that the tumour in the orbit was of a polypous character also. While a third, without expressing any opinion as to the character of the tumour, advised that it should be cut down upon, and any further operation for its removal determined by its nature and connections.

The patient was admitted into the Northern Hospital, and the tumour was removed soon after by Mr Banner. On cutting through the integuments the surface of the tumour was discovered of a white silvery appearance; it was firmly adherent to the lower margin of the orbit, and to the ascending process of the superior maxillary bone, but it did not adhere to the globe of the eye, nor did it appear to have any connection with the cavity of the nostril. The operation was tedious and painful, and attended with a profuse hæmorrhage. The tumour was at last torn from its attachments with the fingers, bringing with it several small fragments of bone, apparently portions of the orbit. The finger of the operator was passed deep into the orbit without discovering any tumour behind the eyeball or in any other situation. The globe of the eye did not resume its natural position after the removal of the tumour.

The tumour when removed was about the size of a small plum, firm, dense, and elastic, inclosed apparently in a capsule. On being cut into it exhibited a whitish yellow surface and a consistence somewhat approaching to that of fibro-cartilage. Attached to its upper part was a reddish substance of irregular figure, like the remains of a polypus, in the texture of which was deposited portions of the same whitish-yellow matter, which formed the bulk of the tumour.

The result of this case was most unfavourable. The displaced eye never resumed its natural position, and vision with the other became so defective that the patient was unable to recognise me when I last saw him. He quitted the hospital some weeks after the operation in this pitiable condition.

Regarding the further progress and fatal termination of this case, I received the following particulars from Mr Wilson, a surgeon at Rundon, where Bent died. His letter bears date about two years subsequent to the operation, and contains the following statement.

"At the time of his death the tumour was equal in size to a child's head, occupying the entire face above the mouth, but inclining more to the right side. There was not a trace of the right eye remaining, and the left was pushed quite out of the orbit, forming the extreme point of the tumour on that side. The nose was also displaced, being flattened and expanded before the front of the tumour. The external surface of the tumour was lobulated, but entire, without any ulceration."

Mr Wilson does not mention the date of Bent's death, but I presume that it must have occurred not long before he wrote to me. From some misunderstanding with the relatives of the patient a *post mortem* examination was not obtained.

Mr W. thinks that the tumour was of an encephaloid, and probably originated in the *antrum maxillare.*

I mentioned the case to Dr Mackenzie of Glasgow, when in that city some time ago, and he was of opinion that it had been a polypous growth from the antrum. In confirmation of this view of the case, which is probably the correct one, I may mention that the disorganization which it appears to have ultimately produced is not greater than that observed in some of the examples of polypi of the nose and fræ nal cavities, so graphically described by Mr John Bell. (Principles of Surgery, vol. iii. chap. 5.)

More recently a case somewhat similar to the above came under my notice. The subject was a young watchmaker, who came to me complaining of dimness of sight in the right eye, and pain over the supercilium. On examination I found the right eye slightly more prominent than the other, and an obvious fulness existing beneath the superciliary side extending the whole length of the eyelid. Pressed with the point of the finger this swelling communicated an elastic doughy sensation, especially towards the inner canthus, where firm pressure caused pain. There was also pain on pressure over the inferior orbital foramen. On pressing the finger under the supercilium of the other eye, the sensations communicated were wholly different. The swelling had been first noticed two months previously, and he did not think that it had increased, nor did it vary during the twenty-four hours. He had suffered from polypi into the nostrils during three or four years past, and had undergone about thirty operations for their removal in the left nostril, which was usually completely stopped unless after an operation. From the right nostril they had been removed for the first time about a fortnight before I saw him, and the passage was then clear.

This youth unfortunately left the town soon after, and I have neither seen nor heard any thing of him for more than a year.

I have also seen a third case more recently still, in which there existed the same elastic doughy swelling beneath the supercilium, attended with slight pain; but in this instance vision was not affected, and there were no polypi in the nares, nor any ostensible cause for the above symptoms. The patient was a young girl from the country, whom I saw only once, and have lost sight of for six months past.

Remarks.—With regard to the *first* case narrated, had the symptoms more distinctly indicated the real seat of the tumour, the most effective practice would certainly have been to have opened the antrum through the cheek, and extirpated the growth at its origin. The ill success which attended the operation actually performed probably deterred the patient from seeking medical advice again till the disease was so far advanced as to preclude all idea of interference with the knife.

The protrusion of the eye and dimness of vision in the *second* case were, I have no doubt, caused by the pressure of polypous growths; but the extent of their origin in this instance was nearly as obscure as in the preceding case, and any operation further than what had been already practised did not appear to be either necessary or warrantable.

The nature of the *third* case I cannot pretend to determine decisively; but, viewed in connection with the two preceding, it presented at least a suspicious aspect, and was well worthy of being closely watched.

2. *Case of Uncircumscribed Gangrene of the Lung.* By JAMES TURNBULL, M. D., Physician to the Northern Hospital.

——— Meteham, a carter, 18 years of age, of rather delicate constitution, was admitted into the Northern Hospital on the 27th October 1845, with cough, and extremely fetid breath, and expectoration.

He stated that he had not been quite well since the middle of April preceding, when he had an attack of pleurisy in the left side, for which he was bled. Since then he had never been perfectly free from cough, and had, "at odd times," expectorated a little blood. He had, however, been able to perform his ordinary work as a carter, until three weeks before admission, when he was seized with pain in the left side of the chest, cough, and expectoration of fetid matter.

When admitted he had a very pale, anæmic appearance, and was somewhat emaciated. There was not, however, much prostration of strength. The breath was very fetid, and the expectoration of a dirty greenish colour, at times mixed with blood, and excessively fetid. The pulse was 124, small; the skin of the natural temperature; and the tongue nearly clean. There was not much appetite, and but little thirst. The bowels were not purged.

Physical Signs.—On percussion there was complete dulness at the lower part of the left side of the chest posteriorly. During an ordinary inspiration, there was heard only feebleness of the respiration, with slight mucous rale; but during a full inspiration, it seemed as if some part, which acted as a valve, were raised, and then the air entered a large cavity, and caused a loud gurgling noise. This sound was heard loudest close to the spine on the left side, but it could also be perceived in the right side of the chest. The voice and the cough were also distinctly cavernous. Anteriorly at the lower part of the left side there was only some mucous rale. Below the left clavicle the respiration was clear, and there was no dulness. The respiration on the right side was puerile.

Diagnosis.—Gangrenous excavation of the lower and posterior part of the left lung.

Quinine and sulphuric acid were prescribed, and chlorinated soda for inhalation. On the 29th, he complained of more weakness, and the fetid expectoration was mixed with a considerable quantity of blood. The physical signs of a cavity were more distinct, and cavernous respiration could be heard during an ordinary inspiration. Some creasote with compound tincture of camphor and oxymel of squill were ordered for him, with nourishing diet and porter. He got gradually weaker, however, and died on the morning of the 31st.

Inspection.—There were old adhesions on both sides of the chest. Those of the left pleura were very firm, and in attempting to separate them, the fingers burst into a large cavity near the lower part of the lung, and the position of which, close to the vertebral column, explained the fact of the signs of a cavity being heard on the right as well as the left side of the chest. The walls of the cavity had in some parts a sloughy greenish-gray appearance, and in others were black. There was no line separating the gangrenous part. The lowest portion of the lung was in the state of purulent infiltration. The condensation of the middle part resembled red hepatization, and the summit was livid red, and contained a sero-sanguinolent fluid, as if in the first stage of inflammation. There were no tubercles, and the right lung and the heart were healthy.

This case is one of uncircumscribed gangrene, which is the rarest form of the disease. It presents the characteristic symptoms of gangrene of the lungs, viz. fetid breath, very fetid greenish dirty expectoration and hæmorrhage from the lungs. These symptoms being combined with the physical signs of a large cavity, left no doubt as to the nature of the disease. The prostration of strength was not, however, so great as might have been expected from the severity and duration of the disease. Fetor of the breath is observed in the circumscribed as well as in the uncircumscribed form of the disease, when the part communicates with the bronchial tubes. It cannot, however, be regarded as diagnostic of the disease, as it occurs in phthisis, where the walls of a tubercular cavity are attacked with gangrene, and likewise in some cases of chronic bronchitis with dilatation of the air tubes.

As regards the origin of the disease it may be observed, that its uncircumscribed character, and the appearance of the surrounding parts, would favour the opinion that it was a termination of pneumonia in a weak constitution. On the other hand, it might be thought, from the occasional expectoration of blood, that the disease was produced by pulmonary apoplexy, which Dr Addison considers the most common cause of circumscribed gangrene of the lungs.

3. *Tubercular Disease of the Lungs; cavities lined with false membrane; pneumonia.* By James Turnbull, M. D.

A Maltese seaman, of muscular frame, and about 40 years of age, who had recently arrived from New Orleans, was admitted into the Northern Hospital on the 21st of November 1845, with extreme difficulty of breathing and very feeble rapid pulse. Scarcely any information could be obtained from the patient beyond the facts, that he had suffered more or less from cough and bronchial affection for about ten years, and that the acute attack had come on three weeks previous. His exhausted condition did not admit of an accurate examination of the chest, and it was concluded that pneumonia or acute bronchitis had supervened upon the chronic form of the latter disease. Wine was prescribed for him with a stimulant expectorant, and a sinapism to the chest. He died, however, the same evening.

Inspection.—There was extensive emphysema of the left lung; the upper lobe especially was very emphysematous, and at the part where it was most so it was puckered in a remarkable manner. On cutting into this emphysematous puckered part, a cavity the size of a walnut was discovered completely lined with firm false membrane and containing purulent matter. Tubercular depositions were found around it, and in the lower part of the lung, in various stages, from the miliary tubercle to the formation of small cavitities. The lower part of the left lung posteriorly was very much congested with dark blood, but became florid on exposure. There was extensive and in some parts old adhesion of the right pleura. At the lower part, between the lung and diaphragm, the adhesion was recent and the false membrane very vascular. The lung was emphysematous, and the upper lobe completely consolidated by inflammation. The summit was in the state of red hepatization, and lower down there was purulent infiltration. At the apex there was a cavity lined with false membrane. There were tubercles in various parts of the lung, and a cavity in the central lobe. The mucous membrane of both lungs was very red and vascular. The heart was rather flaccid. There was commencing atheroma of the aorta, but the valves were healthy.

In this case there are three points worthy of notice, viz. the long period of ten years, during which the disease seems to have been making progress, without the patient being unable to follow a laborious occupation; the effort made by nature to repair the ravages of the disease by lining the cavities with false membrane; and the coexistence of a great amount of emphysema with the tubercular disease; the former being most marked in the situation of the oldest cavity.

4. *Cases of Phthisis with scrofulous degeneration of the Kidneys.*

By Thomas Inman, M. D.

Margaret Hill, aged 29, (April 1843,) married, without children, had sore throat and periostitis some months ago, was admitted into the Infirmary with the characteristics of phthisis. The symptoms presented no peculiarity. No pain was complained of in the loins, and attention was never drawn to the kidneys or to the character of the urine; no dropsy had at any time occurred.

Inspection—disclosed cavities in the upper lobe and crude tubercles in the lower lobe of the left lung, and condensation of the upper lobe of the right; the left kidney was greatly enlarged and healthy looking; the right was small, strongly adherent to the fatty tissue around; its surface was irregular and knobby; the protuberances were found to be caused by a number of cysts containing a fluid of a urinous appearance. In other parts were three cysts containing a caseous substance of a soft consistence. These were situated in the cortical part, and were found on further investigation to be lined with a distinct but rough membrane, whose free surface was studded with tubercle.

Janet C. aged 30, married, had cough for some months before confinement. Six weeks after it was admitted into the hospital with abdominal tenderness, tympanites, and extreme debility, and died in two days afterwards.

Lungs studded with miliary tubercles, and densely gorged with serum.

Peritoneum everywhere studded with small greyish white tubercles of various sizes, which had given rise to inflammation and exudation of false membrane. Kidneys large; especially the right, weighing ten ounces.

The left contained a large scrofulous abscess filled with serous purulent matter and flocks of lymph. The cavity was lined by a distinct membrane, consisting of a pavement of regular scales more or less rounded and containing a central nucleus.

The uterus was about the size of a shaddock, but of ordinary density and appearance.

The liver, though it had not the usual appearance of fatty degeneration, was proved by the microscope to contain an excessive amount of oily matter in its cells.

The aorta contained numerous small spots of incipient atheroma.

A. B. aged 34, female, unmarried, died with the usual symptoms of phthisis; she was under observation a very short time only, but never complained of any symptoms referrible to the kidneys.

The urine was not examined. Dropsy had never made its appearance. These particulars were obtained after death; as I saw the patient in the dead house.

Inspection.—Lungs contained large caverns in the upper part and abundance of miliary tubercles in the lower part.

The kidneys were of immense size, one weighing twenty-two, the other nine ounces; their specific gravity was 1012 and 1024 respectively. The former on section was found to contain very large deposits of cheesy matter; most abundant at the apex and in the cortical substance; a vascular ring existed round each; some of the pyramidal bodies had entirely disappeared. The pelvis and ureters were in a state of scrofulous ulceration, but not impervious. The left kidney consisted entirely of a mass of scrofulous abscesses, separated from each other by a dense white tissue, in which no trace of the true renal structure could be found. The ureter was impervious, and the pelvis converted into a large abscess. The contents of these abscesses were milky serum mixed with innumerable flakes of lymph.

The bladder was healthy.

Liver of the usual appearance. Along the *margo acutus* were a number of minute hard bodies, like the Pacchionian corpuscles, which had excited inflammation around them. The cells of the liver contained scarcely any oil globules.

Aorta contained many spots of the yellowish white colour of incipient atheroma.

Mary Tynan, aged 21, servant, states that fourteen months ago she fell from a window, bruising her side and back, since when she has suffered from constant pain in the lower part of the spine. In about seven months she lost the use of her left leg and foot, sensation partially remaining. About two months afterwards the spine was observed to be gibbous, and an abscess pointed on the left of the umbilicus. Subsequently the opening healed, and the matter pointed on the opposite side. An incision was made, and a purulent discharge continued during the next six weeks. It then suddenly ceased, and she was seized with severe pain in the left side and urgent dyspnœa. It was noticed too that the stools contained a large quantity of blood and matter. Shortly afterwards the pain in the right leg, of which she had previously complained, became excessive, but that in the spine diminished. Both legs were now paralytic, and though painful, not sensible of pressure or irritation, and always cold.

There were no symptoms to call attention to the kidneys, and the urine was not examined. The nurse stated that she passed it freely, that it was sometimes white, but usually clear and healthy looking. Symptoms of phthisis came on with great rapidity, under which she soon succumbed.

Inspection.—Cavities existed in the lungs on both sides; pneumothorax on the left; the lower portions of both lungs were densely studded with crude tubercles.

Liver was large, pale, mottled, presenting the usual appearance of fatty degeneration. Kidneys were very large, weighing together forty ounces. On section they were found to contain, principally in the cortical substance, large scrofulous abscesses and deposits of a cheesy matter of considerable density. The renal tissue in the neighbourhood of these was infiltrated with a whitish matter, answering the usual description of tubercle, but its natural appearance was not entirely destroyed. There was less vascularity in the neighbourhood of these deposits than elsewhere. The pelvis and ureters were greatly ulcerated, but their mucous membrane had not been entirely destroyed.

The bodies of the fourth and fifth lumbar vertebræ were converted into a brown mass, soft and granular, closely resembling rotten cheese. In this no trace whatever of bone could be found, or of intervertebratal substance. This change had passed through the whole of the bodies, and affected the posterior lamina, destroying completely the lower part of the *cauda equina.* No communication or trace of communication could be found between the diseased spine and the bowels, skin, or any other part.

The small intestines were extensively ulcerated; the large presented appearances differing in no respect from those of dysentery as regards the extent of disease. The ulcers, however, had all the red vascular raised edge of the tubercular variety, and leaving the muscular coat bare on removing the softened tissue in the centre.

The aorta presented many spots of a yellowish white colour under its serous coat, especially at the upper part of its descending portion, which were found to consist of a number of aggregated oil globules, disposed either in rows or clusters.

A microscopical examination undertaken in the three last cases served greatly to increase their interest. In all it will have been noticed that there were cavities and crude tubercles in both lungs; that in one the liver was much as usual, in the others that it was fatty, and that the kidneys were diseased, and the aorta found in an early stage of atheroma in all. It then became a matter of inquiry if the microscopical appearances were the same. On examining a portion of the lung in which the tubercular deposit had produced solidification, or where it was sufficiently distinct to be obvious to the eye, it was found to consist everywhere of round corpuscles or globules closely resembling those of lymph or fibrine. In addition to these, especially in those parts where the deposit was yellowish, a number of large cells were to be found, some containing a number of granules of great minuteness, and others which contained nothing but fat. These existed, however, in far less abundance than the round corpuscles, of which the masses were composed.

But though the ultimate appearance in all was the same, a great difference was observed in the earliest condition of the deposit. In the second case the primary change appeared to be the secretion of a grey semitransparent substance when seen under the pleura, but which when found in the substance of the lungs only evidenced its existence by increasing their density. The points where this existed were readily discovered on making an incision into a healthy looking part, when they projected in a striking manner. On placing a small section of these under the microscope, the ordinary tissue of the lung was seen, but no adventitious substance. A repetition of these observations on the grey granulations, under the pleura, produced the same result. They were semitransparent and structureless.

In the last case the deposit in those parts of the lung which were least affected always appeared to the eye of a yellowish white colour, and of considerable size. On making a section of the tubercle and the tissue round it nothing was to be seen but a number of the round corpuscles in various degrees of aggregation, decreasing in quantity as they were distant from the whitish mass. These appear to have been deposited in the intervals between the air cells, external to them and to the vessels, both of which they compress when their increase has been considerable, the fibrous tissue alone being left in the centre of the tubercle. The deposit in the kidney resembled that in the lungs in all, the cells consisting of an aggregation of large round corpuscles, occasionally altered in their shape by pressure, but never entirely losing their distinctive character.

The mode in which this deposit had taken place was similar to that in the lungs. In the vicinity of a tubercle the renal tissue was whiter and more solid than usual, and on making their sections at this part the urinary tubes were seen comparatively healthy though reduced in diameter. Scattered around and between them, however, were a number of the round corpuscles to which we have so often referred. As these had increased the tissue had given way before them by absorption, and the vessels become entirely impervious.

The earliest microscopical condition of tubercle has been variously stated by different authors, and great discrepancy has arisen, either from their confounding together many different pathological conditions, or confining themselves too closely to one particular point.

From an examination of a considerable number of cases I am inclined to believe that there are at least four different ways in which tubercle is originally developed. Two have been already noticed when speaking of the appearances actually observed. The other two are more uncommon, three instances only having been met with out of about fifty observations. One consists of an aggregation of large cells containing an infinite number of round granules of a brown colour, and uninfluenced by the usual reagents. The size of the cell varies from $\frac{1}{400}$th to $\frac{1}{350}$th of an inch, of the granules $\frac{1}{6000}$th to $\frac{1}{3000}$th of an inch. These cells are sometimes found collected in masses which present somewhat of the characters of encysted abscesses, at others scattered in the substance of the lungs either singly or in groups. This variety evidently answers to that described by Lebert as the usual and characteristic appearance of tubercle.

Of the fourth variety I have only seen one example. A tubercle of a white colour the size of a pin's head was taken from the peritoneum of a woman who had died of peritonitis. On examining this it was found to consist of a number of large cells, rendered dark in consequence of their being filled with innumerable globules, which were proved by pressure to be of an oily nature. A few granules were observed, but owing to the number of oil globules which were floating about, this point could not be satisfactorily determined. On pressure between plates of glass the oily matter was expelled, leaving a hyaline mass behind. These observations were repeated and varied, but always had the same result.

I examined a hard tubercle from the lungs of the patient whose case is recorded by Dr Turnbull, and found some interesting phenomena, which throw considerable light on the pathology of the change.

On making a thin section and floating the knife with water a number of minute air bubbles were seen making their escape from the solid mass, showing that the tissue was not quite impervious. In other parts, where the deposit was greater and more extensive, none of these bubbles were met with. On placing thin slices under the microscope the former was found to consist of air cells, whose walls were thickened and opaque. In the latter nothing was seen but an opaque brownish mass in which the remains of the fibrous tissue was alone to be discovered. In both the deposit had evidently been external to the air cells, compressing them till they were rendered almost impervious. After a while, however, with the absorption of its more fluid parts, its bulk had been diminished, and the pressure being removed the air cells had recovered their patency in the one case.

In the other case the compression had gone on till it had obliterated both the air cells and the pulmonary vessels, rendering that portion of the lung impervious and useless. Had the patient survived a longer time it is probable that this would have become converted into a cretaceous or ossific mass. In all cases where there has been incipient softening I have found it in a point at the greatest distance from the healthy structure, and have every reason to conclude that it is a process strictly analogous to ulceration.

Fatty degenerations are much more common in cases of phthisis than is generally supposed. Dr G. Johnson has already pointed out the frequency of its occurrence in the kidneys, Mr Bowman in the liver. It will also be invariably found in the aorta, where it assumes the appearance of whitish yellow spots under the serous coat. This degeneration is found especially at the upper part of the descending portion; an observation I have verified by the experience of every case examined at the workhouse during the last twelve months.

5. *Rupture of the Left Auricle.* By JAMES VOSE, M. D., Physician to the Liverpool Infirmary.

I am favoured by a practitioner in town with the present specimen of a somewhat uncommon lesion of the heart. The note of the case is as follows: A lady, aged 74, had been complaining for some days of pain at the epigastrium and over the cardiac region; there was some dyspeptic uneasiness, and great mental distress had been for some time experienced, through a severe domestic calamity. The bodily symptoms first referred to subsided in a great measure, and the day preceding her dissolution the patient felt much better. On leaving her at night, the attendant remarked that this lady was free, or nearly so, from uneasiness of any description. In the morning, the patient was found dead in bed. She was lying in a supine posture, the limbs were straight, and the body quite warm.

Dissection, 24 hours P. M. Upon slitting up the pericardium about three ounces of bloody serum gushed out, while a layer of coagulated blood was found spread over nearly the whole surface of the heart. When this organ was removed and examined, a large opening was discovered in the walls of the left auricle; the form of this opening was nearly circular, and its edges were soft and somewhat ragged or fringed.

The substance of the heart generally was thought to be a good deal softened.

The circumstances which in this case appear to be most interesting are, 1*st*, the great depression of mind which, as has been mentioned, existed for some time before death; 2*d*, the large size of the rupture in the heart's walls; and 3*d*, the situation of the lesion, which was not, as has been usually observed, in the substance of a ventricle, diseased by atheromatous or earthy depositions beneath its lining membrane, but directly through the parietes of an auricle, which does not appear to have undergone any appreciable form of morbid degeneration, for the softness observed does not seem to be more than is again and again found in hearts which have not given way.

Morgagni, Portal, and Corvisart mention that they had seen examples of rupture of the heart, and that these lesions were usually in the left ventricle. These authors considered violent exertions, gusts of passion, and the struggles of the epileptic paroxysm, to have been, in their experience, the most frequent causes of these accidents. Dr Whytt saw a heart which had burst from excessive grief; and we can hardly venture to impugn the accuracy of a statement coming from so eminent an observer.

Hope remarks that, in a few instances, rupture of the heart appears to have resulted from a strong mental emotion; but while he thinks that such an accident could scarcely arise from such a cause, without previous disease of the muscular substance, he admits that sometimes no appreciable morbid state can be found in the situation where the perforation exists. In the case of George II. death occurred instantaneously, and the walls of the left ventricle were found rent. Dr Baillie, when writing towards the end of his career in 1817, says that he had only seen one example of rupture of the heart.

In the case of a late eminent physician, whose loss the public and the profession will long deplore, death appears to have taken place instantaneously, from the giving way of some of the external fibres of the left ventricle, and the consequent effusion of blood into the pericardium, from ruptured coronary arteries and veins. The heart's action was mechanically arrested by the pressure of the extravasated blood which was found around it, clotted, and in great quantity. In this case the texture of the heart was much softer, and its substance was more loaded with fat than is natural. The aortic valves, the aorta itself, and the coronary arteries, were marked by atheromatous spots. It is added that a bristle was introduced into the ruptured orifice of a considerable coronary vein, and easily passed up towards the base of the heart.

I may also mention, as connected with the present subject, the case of a man who fell down in the street in this town, a few years ago, and was conveyed to the Northern Hospital, where he very shortly expired. Upon dissection, a large quantity of dark, clotted blood was found in the pericardium, and it was discovered that this hæmorrhage had been furnished by a vein of considerable size, which ran along the obtuse margin of the heart. The mode in which the blood had made its way into the pericardial sac was singular; a small laceration had taken place in the coats of the vein. The blood escaping from this had insinuated itself between the investing layer of the pericardium and the heart, raising and stretching the membrane to the extent of about half an inch. It had caused a minute rupture of its texture, through which it poured itself over the surface of the heart. To conclude. Of ten cases of ruptured heart mentioned by Bayle, eight died instantly, one in about two hours, and another in fourteen.

6. *Cases of Chronic Pericarditis.*

Great obscurity appears to have involved, at all times, the diagnosis of chronic inflammation of the pericardium ; and even since morbid anatomy and the art of auscultation have received zealous cultivation at the hands of so many members of the profession, it will probably be very generally admitted that a considerable proportion of these cases continue to be a source of much embarrassment to the practical physician, being often overlooked altogether, or confounded with diseases having a widely different origin. As the subject is one of much interest and importance, and as well marked cases of the disease are not of very common occurrence, I may, perhaps, be excused for trespassing on the time of the Society with a short account of the symptoms, and appearances observed upon dissection, in three examples of chronic pericarditis which have latterly been under my care—and having related them, I shall venture upon an analysis of the principal points of practical value which their history suggests, and offer a few general remarks in connection with these.

Case 1.—A man, aged 60, came under my care with the following symptoms: His countenance had a remarkably sallow and cadaverous appearance; his breathing was hurried; his pulse small and irregular. There was no pain in the chest; the sleep was tolerably calm; some œdema. In a short time, without any important variation in the symptoms now enumerated, this patient's strength rapidly gave way, and he expired.

Dissection.—Upon puncturing the pericardium, much clear serum escaped. There was great thickening of the membrane, from interstitial deposition, and its surfaces were roughened by ridges and tufts of effused fibrin and adventitious structure, the "neat's tongue" appearance being well shown by these parts. Numerous tubercles were scattered through the lungs, and several ulcerations existed along the intestines.

In this case, as there were neither cough nor diarrhœa present, to lead to the suspicion of the pulmonary and abdominal changes which were found upon dissection, my attention was especially directed to the condition of the organs of circulation; and I was of opinion, upon first seeing the patient, that he was affected with structural disease of the heart; the dyspnœa, the peculiar character of the pulse, and the œdema suggesting this opinion; but there was no pain or tenderness at the *scrobiculus cordis*, or over the cardiac region, or, indeed, any special symptom indicative of an altered state of the pericardium.

Case 2.—A gentleman, aged 25, came under my care in May 1845. The leading symptoms were restlessness, anxiety, sighing, dyspnœa, cough, with an occasional appearance of blood in the sputa; the pulse was small, frequent, and very irregular; there was some lividity of the countenance, and a slight tendency to œdema of the feet was observable. The dulness upon percussion over the cardiac region was much more extensive than it is found to be in health; the impulse and sounds of the heart were feeble and remote; but no murmurs accompanied the latter. Much pain was complained of when pressure was made upon the *scrobiculus cordis*, as well as when it was directed upwards from the margin of the ribs upon the left side; but pressure over the sternum or costal cartilages to the left of it did not appear to cause suffering.

Upon investigating the history of this case, I found that the patient had been in the habit of violently exerting himself in rowing matches, that he had been under a trainer to qualify himself for this amusement, and that, from his great muscular strength, he was accustomed to court fatigue and exposure. His health began to decline during the winter, but until the spring he continued to attend somewhat to business. It was not until the early part of May that he was obliged to place himself strictly under medical care.

From a review of the circumstances in this case, I felt well assured that the patient was labouring under chronic inflammation of the pericardium, and that there was an abundant collection of fluid in the sac of the membrane.

Much mitigation of suffering was derived from a regulation of the diet, by laxatives, and by the use of æther and morphia draughts at night. Mercury and opium were given, in free doses, every fourth hour for many days; but no effect of any description was produced by these medicines. Towards the last few days of life, the abdominal parietes became appreciably anasarcous on the side on which the patient most commonly reclined.

Death appeared to take place by syncope, and for many days before dissolution, pressure over the epigastric and left hypochondriac regions no longer gave any pain.

Dissection.—Large quantities of pale serum escaped when the chest was opened. Upon raising the sternum and the annexed costal cartilages, the pericardium was observed to occupy the whole of the fore part of the thorax; extending much to the right of the sternum. The membrane was greatly thickened, and on puncturing it a copious discharge of clear liquid took place; there was no appearance of flakes or particles of lymph in this.

The inner surface of the pericardium and the outer surface of the heart were abundantly coated with pseudo-membranous deposition. This was so arranged as to present an appearance, partly of coarse, flocculent shreds, and partly of mesh work. No band passed between the heart and the pericardium, except near the base of the former. In that situation there were numerous short, thick processes of adventitious structure, running from one serous surface to the other.

CASE 3.—A man, about 30 years of age, was under my care a few months ago. He laboured under a complication of disorders; but his principal symptoms were cough, dyspnœa, anasarca, and albuminous urine. I suspected cardiac disease, but catarrhal rales of such loudness were present, that I could never satisfy myself as to the character of the sounds which accompanied the heart's action. The most distressing symptom in this case was the dyspnœa, which, although never absent, became every now and then so extreme as to throw the patient into an agony of suffering. At these times, the breathlessness assumed a very peculiar character, coming on in fits or gusts. The patient died in June, worn out with the severity of his disease.

Dissection.—The air tubes exhibited the usual appearances which are observed in severe bronchitic cases. The layers of the pericardium were intimately connected to each other, by means of fibrinous exudation, with which they were encrusted. The pericardial sac was, in fact, obliterated, so closely were its surfaces in contact. The walls of the left ventricle were thickened, and its cavity was dilated; but no appearance of valvular disease presented itself.

Was the eccentric hypertrophy observed here—the result of an extension to the heart's substance of the stimulus of the inflammation which had existed at the pericardium—or did it arise from the exaggerated movements which the fibres of the left ventricle must have had to perform to overcome the embarrassment which the adhesion of the pericardial surfaces to each other had doubtless occasioned them? Either of these explanations is admissible, in the absence of any valvular changes to account for the hypertrophy.

When we glance at the foregoing cases, we are struck by a certain similarity among several of the most important symptoms by which they were characterized. Thus cough was present in all; yet in consequence of the tuberculated condition of the lungs in one of the patients, and the obvious state of bronchitis in another, we cannot, with any propriety, connect this symptom with the pericardial disease. Again, dyspnœa, to which so much importance is attached by the most careful observers, as a pathognomonic indication of pericarditis, was present in all the three patients; but here, as in reference to the cough, the pulmonary mischief which existed in two of the cases, invalidated the significance of the dyspnœa as a symptom of inflamed pericardium. In all the patients, the breathlessness was the most distressing circumstance that presented itself.

In two of the cases, the pulse was small and irregular. The countenance of one patient was remarkable for its anemic sallowness; that of another was slightly plum-coloured at the lips and cheeks; while the face, in the third case, was swoln and congested. Hæmoptysis was present in one of the cases only, viz. in that in which there was no reason to suspect disease of the lungs. Œdema was observed in all; but as in one the urine was notably albuminous, no stress can be laid upon the anasarca as being characteristic of pericarditis in that instance. In only one of the cases was there any unequivocal complaint of pain, upon pressing the epigastric and left hypochondriac regions. The friction sounds which are so often heard to accompany the heart's movements, when the surfaces of the pericardium are encrusted with lymph, and which, when present, are of such very great value in enabling us to diagnosticate the existence of pericarditis, were not audible in any of these cases. The appearances on dissection explained their absence; for in one instance, the surfaces of the pericardium were intimately adherently solid, and in the other two, they were widely separated by liquid effusion, as regards the anatomical changes. It is said by Baillie that the fibrin thrown out in pericarditis has an appearance of fine net-work or lace. Others again, among whom the late Dr Hope may be especially cited, speak of the fibrin as presenting an alveolar arrangement, most commonly; and figures are given in his work on morbid anatomy illustrative of this, as well as of examples of the disease in which the exuded lymph has a worm-eaten aspect, or presents a rippled surface resembling sand upon the shore.

It will have been observed that, in the preceding cases, none of these appearances were present. In one the fibrin was laminated or pellicular; in another it was loose, flocculent, and arranged in coarse meshes; while in another the pericardium strikingly resembled a neat's tongue, from the lymph smoothly encrusting it in some places, and appearing in the form of fine tufts and ridges at other points.

7. *Aneurism of the Cœliac Axis.* By James Turnbull, M. D.

A sailor, 23 years of age, pale and thin, came under my care in the Northern Hospital in the beginning of June 1845, with a pulsating tumour in the epigastrium. He had recently returned from a voyage to the East Indies, during which he had an attack of dysentery, and he first perceived the pulsation at St Helena when recovering from it. He could assign no cause for the tumour, but thought that it had been produced by the dysentery. The tumour was situated just under the cartilages of the false ribs on the left side. It was distinctly circumscribed and had a strong pulsation. When the stethoscope was applied to it a loud bellows' murmur, having a hoarse aneurismal character, was heard during the pulsation. It was audible also along the aorta, but diminished in intensity in receding from the tumour. The sounds of the heart were quite natural, and the pulse was 76. He complained of severe pain in the tumour increased by examination, and had at one time been unable to lie down. He had also sickness and vomiting, which were relieved by hydrocyanic acid, with digitalis, and by muriate of morphia.

On the evening of the 5th of June he was seized with agonizing pain in the abdomen, collapse rapidly followed, and he died about 20 minutes after.

Inspection.—The abdominal cavity contained a large quantity of coagulated blood, which had oozed out from a small opening in a tumour, which at first appeared to be an aneurism of the aorta. The tumour was as large as a moderate sized apple, and was found to be an aneurism of the cœliac axis, the three branches of which were seen coming off from its anterior part. On its anterior several layers of fibrin were deposited. The heart, the valves, and the coats of the aorta seemed healthy.

8. *Case of Scirrhous Ulceration of the Œsophagus communicating with the Trachea.* By J. P. Harris, Esq. Surgeon to the Eastern Dispensary.

Henry Noble, aged 56, labourer, of good conformation, sanguineous temperament, states that he has always enjoyed good health, until about three years ago, when he had an attack of synovitis of the knee-joint, which ended in ankylosis. He has been and is accustomed to drink freely of ardent spirits, though irregularly.

About a week before Christmas 1844 he experienced some difficulty in swallowing, especially solids. He was not led to seek assistance until a month subsequently, when, after three or four days' indulgence in drinking rum, the difficulty of swallowing returned, attended with pain at the epigastrium. He applied for relief at the Northern Hospital, (where he was treated by the daily use of bougies,) and left at the end of a week, seemingly improved, his food meeting with little obstruction in its passage to the stomach. The relief, however, appears to have been of short duration, as a few days after he experienced a relapse, in which instance he does not appear to have sought medical aid. He continued to suffer from the recurrence of the above symptoms for the space of two months, when he applied to the Eastern Dispensary.

His symptoms were as follows :—Great emaciation ; distress of countenance ; sense of burning and constriction referred to the œsophagus ; cough, with the occasional expectoration of a kind of tenacious mucus, tinged with blood ; pain and a sensation of sinking referred to the epigastrium, the result of prolonged abstinence from food ; bowels confined, have not been relieved for fourteen days; headach; dimness of sight; great thirst; pulse 90, small and thread-like; temperature of surface much reduced; urine scanty. Percussion yields a tolerably clear sound over the chest, at the upper part of which the respiration is bronchial.

Every attempt to swallow food (which consists entirely of fluids in very small quantities) is attended with much suffering. The food seems to pass no lower than about the middle of the chest, and after being retained for a time, varying from a few seconds to two or three minutes, is rejected by an inverted action of the œsophagus, and the small quantity which seems to reach the stomach is frequently rejected by vomiting; the matter discharged has an offensive smell; the patient fancies his food takes some unusual course besides that to the stomach.

An elastic tube was with little difficulty passed to the middle of the œsophagus, but any attempt to get it beyond excited so much distress as to compel its disuse. Nutriment was administered by the rectum, which sustained life for three or four weeks, though in a miserable condition, when he appeared to sink from the effects of inanition.

Sectio cadaveris, fifty-six hours after death. The apices of the lungs were thinly studded with tubercles, varying in size from a millet seed to a horse bean; the bronchial tubes were filled with a frothy mucus, and their lining membrane slightly congested; the glands at the bifurcation of the trachea were mottled in appearance, hardened, and much enlarged. On tracing the œsophagus to its middle, it was found very firmly adherent to the spine and surrounding tissues for three or four inches of its course down to its passage through the diaphragm, and to have undergone complete scirrhous degeneration, passing into the ulcerative or carcinomatous state; the surrounding cellular tissue was of a cartilaginous hardness; the aorta, though adherent, seemed free from disease. The trachea was closely united to the diseased gullet, and communicated with it by an ulcerative opening, large enough to admit a good sized catheter; the bodies of the dorsal vertebræ and intervertebral cartilages had began to be involved in the disease; the heart and abdominal viscera were much paler and thinner than usual, but otherwise healthy.

9. *Case of Perforation of the Stomach.* By JAMES TURNBULL, M. D., Physician to the Northern Hospital.

An unmarried woman, 25 years of age, a domestic servant, of stout habit, but with a dark circle round the eyes, was suddenly seized with acute pain at the stomach, followed by some sickness and vomiting. She had been long subject to pain at the stomach, especially after taking food, and the catamenia had been very irregular, appearing generally at longer intervals than a month. She had taken tea and was going up stairs when the pain seized her.

When seen a few hours after by my friend Mr Hutchinson, she was cold, with feeble quick pulse, and had tympanitic distension of the abdomen. Carbonate of ammonia and laudanum were at first prescribed, and afterwards, when she revived, a pill was given every second hour containing one grain of opium and one of calomel. A turpentine enema was also administered.

On the afternoon of the following day, when I first saw her, there was pain of the whole abdomen and tympanitic distension; the skin was warm and perspiring; the pulse 130, small; and the tongue moderately coated and moist at the edges. There was much thirst. She was perfectly sensible, and no narcotic effect had been produced. She was directed to continue the pills every third hour, to remain perfectly quiet, and to take only small quantities of cold water.

On the 1st of February she had been taken out of bed and felt more pain in consequence; pulse 120. In the afternoon she seemed worse. The pulse was 110, small, weak, and fluttering. The pills were continued, and she was allowed a little tea or gruel. On the 2d she had sunk down in bed. The abdomen was very tense, but the pain was not very severe. She was rather sick, and wished to have a purgative or an emetic; pulse 110. On the 3d she seemed rather better, and the pulse was 120 and fuller. On the 4th she had again sunk down in bed and seemed much worse. The pulse was 130 and small. She had continued the pills, but the gums were unaffected. She died at 6 o'clock the same evening, just five days from the time the pain had seized her.

Inspection.—The peritoneal cavity was filled with gas, but it contained no fluid. The peritoneum covering the intestines was red and moderately inflamed. The liver was found overlapping a perforation of the stomach situated near the œsophageal opening on the anterior surface. The perforation was the size of a large quill. The mucous coat was destroyed to a greater extent, and the aperture in it was larger than in the others. A puckered cicatrix from an old ulcer was also observed in the small curvature. The uterus was healthy. The ovaries seemed small and atrophied. The Fallopian tubes were vascular.

10. *Perforation of the Ileum.*

A very robust seaman, about 35 years of age, was carried into the Northern Hospital in a state of collapse, with the pulse almost imperceptible. He had been ill three days, and had been drinking to excess. He complained of pain in the lower part of the right side of the abdomen, and had suffered from vomiting.

The house surgeon went out of the ward to bring some brandy for the patient, and in the meantime he staggered across it to the water-closet. At the door he became so weak that two of the other patients went to his assistance. He dropped down, however, upon his knees while in their hands, and was dead in about ten minutes after he had been brought into the ward.

Inspection.—In the right iliac region a quantity of purulent lymph was found effused. About the middle of the ileum or rather higher, there was a rounded perforation nearly the size of a sixpence. The edges were rather red, and slightly thickened. The mucous membrane and the glands of Peyer seemed healthy. The stomach contained a considerable quantity of half-digested food.

Remarks.—The first of these cases presents the ordinary characters of those of spontaneous perforation of the stomach, which occur generally in young unmarried women, in whom the catamenia have been irregular, though in other respects the patients may have appeared healthy, and may not even have previously complained of pain at the stomach. The chief point of interest here is the length of time that life was protracted after the occurrence of perforation, a fact perhaps attributable to the liberal use of opium, on which, with perfect rest, our reliance should in such cases be chiefly placed. Whether calomel should be at first administered or not is a questionable matter. It has an undoubted power of controlling the inflammation of serous membranes; but on the other hand it might, if early salivation were induced, prevent the effusion of lymph by which alone the perforation can be repaired. We should probably, therefore, act most judiciously in such cases by relying upon the liberal use of opium alone for two or three days, and then (if life continued) by combining calomel so as slightly to affect the mouth. Death seemed to have occurred here from the depressing effect of the inflammation combined with the embarrassment of the respiration and circulation from the great abdominal distension.

The second case is one of a much less common description. Perforation of the lower part of the ileum occurs not very unfrequently in the course or towards the termination of continued fever and in phthisis. Cases, however, like this, where, as far as we can judge from the imperfect history, the patient seemed to have been in perfect health, are of much rarer occurrence, and appear most nearly allied to those of spontaneous perforation of the stomach. One somewhat similar to this is related by Dr Abercrombie, where the perforation was situated in the duodenum.

11. *A Case of Rupture of the Stomach from the rapid develope-*

ment of gas, as a consequence of acetous fermentation of its contents. By James Carson, M. B.

I give the particulars of this case in the words of Mr Parker, at that time House Surgeon of the Northern Hospital.

Evan Thomas, aged 20, a sailor, of temperate habits, admitted July 3, 1844, under Dr Carson, states that he felt some griping pains in the abdomen after dinner, and that they soon went off. At 6 P. M. he made a hearty meal of coffee with bread and butter, his master partaking of the same food. Shortly afterwards he began to suffer intense pain in the abdomen, which was constant, but somewhat increased at intervals. Some brandy was given to him, and afterwards some medicine from a druggist, but neither affording any relief. At 9 P. M. he was brought to the hospital. He seemed to be suffering much, and rolled about the floor in agony. The abdomen was hard, contracted, and tympanitic, not much swollen, but tender on pressure; countenance anxious; features collapsed; surface of the body cold; pulse small and feeble; retching, no vomiting. His bowels had been open during the afternoon. *Habt. Vin. Opii, Tincturæ Hyoscyam.* āā ʒss.; *Mixt. Cumph.* ℥i. A warm-water injection was given, but soon after returned without fæces.

11 P. M. No better; the pain constant and excruciating. Decubitus on the left side with knees drawn up. Occasionally he sits up in bed and rotates his body, to show, as he says, that there is something loose in his belly. A sound is produced as if of fluid in the intestines; voice strong; tongue slightly dry and furred; vomits occasionally a little fluid; pulse 76, of tolerable strength; passed flatus down without relief. About eighteen ounces of blood were removed from the arm in the erect position. The pulse was sensibly affected by the bleeding, but no mitigation of the symptoms took place. A turpentine injection was given with the long tube without effect. A stimulating draught of ether, assafœtida, and cinnamon was administered without relief. A large blister was then ordered to the abdomen; and a pill containing *Cal.* grs. ij., *Opii*, gr. i., to be taken every hour.

July 3d, 4 A. M. No better; pain intense; abdomen rather more swollen and tender; constant restlessness; vomits occasionally, but throws up only a small quantity of thin colourless fluid; no stool; pulse of natural frequency and good strength.

8 A. M. Is evidently much worse; is quite sensible, and thinks he is dying. The pain has continued unabated; abdomen greatly swollen, as hard as a board—(no impression can be made with the finger on any part of it)—tympanitic, and motionless during respiration. The thoracic parietes, up to a level with the mammæ, are fixed, protruded, and extremely resonant on percussion; respiration quick, laborious; pulse almost imperceptible; surface cold. In about an hour he died. At the moment of dying ex-

tensive emphysema took place in the neck, upper parts of the chest, particularly under the breast, and extended down to the abdomen.

Inspection of the body was made at three o'clock upon the same day. The emphysema has extended to the cheeks, which are puffed out, and down to the lower parts of the abdomen. The two lower wards of the chest are greatly distended and as hard as marble. The abdominal parietes are tense, and on being percussed sound like a board. On opening the peritoneal cavity an immense and forcible escape of gas took place, and the parietes at once became flaccid. The cavity contained a quantity of dark green-coloured effervescing fluid, of a very pungent acid smell, which gave a sharp tingling sensation to the hands, after they were for a short time immersed in it. At the lower part of the abdomen and in the pelvis, there was a very large quantity of froth, as if from rapid fermentation. No effusion of lymph or other sign of peritonitis was observed. The stomach was greatly distended, and pushed up the diaphragm to a level with the nipples. On taking hold of its anterior surface and raising it, a quantity of gas escaped from a large opening, capable of admitting three or four fingers, situated apparently near to the pylorus, but which was afterwards found to be at the superior and posterior part of the great extremity, about three inches from the œsophagus. On cutting open the stomach, the whole of the mucous membrane lining the great extremity was intensely injected of a deep red colour, much softened in texture, and covered with a thin layer of reddish mucus. The edges of the opening were regular, as if torn from overdistension of the viscus. The stomach contained several ounces of pultaceous matter. The intestines were not much distended. The whole of the mucous membrane of the small bowels, from the stomach to the cœcum, presented one continuous appearance of deep-red injection resembling velvet. In some places slight extravasation of blood had taken place. A number of round worms (*L. teres*) were found, and when turned out several of them were alive. The mucous membrane of the great bowel presented the same appearance, but not so uniformly nor to quite so great a degree. About eighteen inches of the rectal end presented a normal appearance. There was another healthy portion between the cœcum and descending colon. The colon contained a moderate proportion of healthy fæces. The mucous membranes of the œsophagus and pharynx were quite healthy. The bladder was contracted and healthy. The liver, spleen, kidneys, and pancreas presented no abnormal appearances.

Chest.—The diaphragm was pushed up by the distended stomach to a level with the nipples; the lungs were consequently exceedingly contracted, being not larger than those of a new-born child. Numerous adhesions existed on each side, but no laceration could be detected to account for the emphysema of the neck and chest. The heart was small and much contracted. A chemical examination of the stomach, its contents, and of the fluids in the cavity of the abdomen, was made by Dr Brett. The contents were extremely acid from the presence of a large quantity of acetic acid. The slightest possible trace of arsenic was discovered, but so slight that Dr Brett did not consider it sufficient to enable him to give an opinion that arsenic was the cause of death. An open verdict was returned that deceased died of inflammation and perforation of the stomach, which might have been produced by arsenic or by natural causes; which of the two there was no evidence to prove.

The above is a very accurate account of the appearances after death, and, as far as I witnessed them, of the symptoms during life. When I saw the patient at 11 P. M., I recognised the presence of great distension of the stomach by gas. It immediately occurred to me, and indeed I directed that a long tube should be passed by the œsophagus into the stomach to relieve this symptom. I have to regret that indecision, and too slight an apprehension of the dangers that might result from an overdistension of this organ led me to depend upon less efficient means of dispersing the accumulated gas. It is well known that rupture of the stomach often takes place in ruminating animals, from the development of carbonic acid gas, from acetous fermentation of the contents of the paunch after feeding upon fresh clover. But I am not aware of any case on record in which death in the human subject has resulted from gas, the product of acetous fermentation. In the article Tympanites in the Dictionnaire des Sciences Medicales, which is a good summary of the literature of this subject, there are related cases of enormous tympanitic distensions of the intestinal canal, from contraction of some part of the colon. In all these cases, the gases were fœtid, and apparently of the same character as those usually formed in the intestinal canal. In the article Meteorisme in the same work, the practice of relieving great tympanitic distensions of the abdomen by acupuncture is pointed out as a resource in extreme cases. My friend Dr Trench mentioned to me a case of a lady in whom a rapidly-formed tympanites, which threatened suffocation, was relieved by her vomiting up a large quantity of wind and fluid of an acid character, which effervesced strongly in the vessel receiving it. In the systematic writers upon this subject, the presence of worms is indicated as a cause of tympanites, and Sauvages has a species called *Tympanites verminosa.* In the present case a large cluster of eleven lumbrici was found in the lower part of the duodenum. Inflammation of the mucous membrane has also been noticed as a cause of this symptom. Intense inflammation of the mucous membrane of the stomach and small intestines was found in the present case; but from the previous state of health, and from the symptoms observed at the early stage, I apprehend that it ought rather to be considered as a consequence of the affection. I think that, although a slight trace of arsenic was found by Dr Brett either in the contents or in the coats of the intestines, yet the absence of prominent appearances of poisoning by this metal, and more especially the absence of more full evidence of the presence of such a quantity as would produce the symptoms in a patient in whom very slight vomiting or other evacuation had taken place, render it exceedingly improbable that this was the cause of the affection.* The position of the stomach in which the superior and posterior portion of the great extremity was, at a cursory view, taken for the pyloric extremities, indicating a complete twist upon its axis, will account for the absence of vomiting.

12. *Case of Hæmatemesis from Rupture of a Cardiac Branch of the Coronary Artery of the Stomach.*

Leroy Kivey, a sailor, aged about 27, was brought into the Northern Hospital upon Friday, 5th November, about eight in the evening, from his ship, after having vomited a large quantity of blood. He had had a similar attack in the morning. Before my arrival at the hospital, which was at ten o'clock P. M., he again vomited about a quart of red blood, not frothy, and mixed with mucus. I found him lying upon his right side with a pale exsanguine complexion. His face, neck, and shoulders, covered with a clammy perspiration. There was no pain upon pressure at the epigastrium. The pulse was not perceptible at the wrist, and nothing but a slight fluttering movement of the heart was distinguishable by the stethoscope. The expression of his countenance and his attitude indicated great prostration. I ordered for him immediately twenty-five drops of laudanum, half a drachm of chloric æther, and some camphor mixture, and directed that he should have ten drops of laudanum and twenty of chloric æther every hour during the night, and if he should rally a turpentine enema. A mustard cataplasm was applied to the abdomen. On the morning of the 6th I found that he had rallied during the night, and the turpentine enema had been administered, which brought away a considerable quantity of black tarry matter. The pulse was considerably improved. The countenance and attitude were, however, still expressive of extreme prostration of strength. He had vomited again a large quantity of blood of a darker colour mixed with clots. The following mixture was ordered to be administered every three hours:—

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* Among these prominent appearances, I would mention the absence of inflammation of the rectum,—I believe always found in cases of poisoning by arsenic.

℞. *Quin. Disulph.* gr. ii.; *Tinct. Opii* m. v.; *Acid. Sulph. Dil.* m. xii.; *Magnes. Sulph.* ʒi.; *Aquæ Font.* ℥i.

It appears that during the day, until the evening, he vomited the same kind of blood. From six o'clock until three A. M. he was free from vomiting. At this period he had again a return of vomiting of a still darker blood mixed with clots. On going into the wards on the 7th I found two of the convalescent patients arranging what was supposed to be his dead body.

Mr Deane, the house surgeon, coming into the ward shortly after I left it, observed that the man was yet alive. He immediately proceeded to pour in stimulants both by the mouth and *per anum*, by which means he was enabled to rally him considerably. In spite, however, of his exertions the man sunk during the night, and expired the following morning. The body was examined thirty hours after death.

Chest.—The viscera contained within this cavity were perfectly healthy, with the exception that they all appeared drained of blood. There were about four ounces of bloody serum in each pleural cavity. The base of each lung, but more especially of the right, was gorged with bloody serum, evidently from position, and from his having generally, while in the hospital, lain upon his right side. This circumstance is an additional evidence that in fevers and diseases of debility this serous congestion observed after death takes place during life.

Abdomen.—The same anæmatous appearance of all the viscera of this cavity presented itself. The large intestine appeared of a dark colour and much distended, its contents being black and partly digested blood. The small intestines were contracted particularly towards the ileum, where for the course of ten or twelve inches, they would scarcely admit the blunt extremity of the enterotome. Their mucous membrane was perfectly healthy from the pylorus to the cœcum, and they contained a considerable quantity of digested matter, of the consistence of gruel, and of a pale-yellow colour. Neither they nor the stomach contained the smallest portion of blood. The stomach was of the usual size, quite empty. Its mucous membrane was covered with a thick glairy mucus, and was in large patches throughout of a dark chocolate colour. In order to ascertain whether there was any diseased condition of this membrane, I placed it in the hands of Dr Inman, who kindly undertook to examine it with the microscope. From him I received the following account of the appearances, which fully explain the cause of the hæmorrhage.

After macerating the stomach twelve hours, the dark colour almost disappeared. On examining it under a low power, it was found that the colour was due to blood, which had been decomposed in the capillaries, probably from the action of sulphuretted hydrogen. On taking up the organ casually I observed a red spot, from which I found a reddish coagulum slightly projecting. On trying to extract this I found it of considerable length and tenuity. The aperture through which the hæmorrhage took place was small, about the size of a barleycorn, round, and situated about a quarter of an inch from the termination of the œsophageal mucous membrane. On dissection it was found to communicate with the superior branch of the *coronaria ventriculi*, where it anastomoses with one of the œsophageal arteries. The size of the vessel was sufficient to admit a good sized pin's head. With this exception the mucous membrane appeared perfectly healthy.

The liver was of a smaller size than natural, which may be explained by its anæmatous condition. It was not otherwise diseased. The gall-bladder was large and distended with bile. The spleen was large and of a more fleshy structure than natural, quite anæmatous, only a few drops of blood exuding from some of the large veins on its section. The splenic veins were very small and contained little blood. The kidneys and suprarenal capsules were healthy. The kidneys alone presented an exception to the general anæmatous condition of the other organs, the quantity of blood, however, not being greater than natural. The bladder was perfectly healthy, containing about eight ounces of rather turbid straw-coloured urine.

I have never previously had an opportunity of examining a case of hæmatemesis fatal from the effects of the hæmorrhage. It is evident, from the absence of blood in the stomach and small intestines, and from the character of the blood vomited on the Saturday evening and the Sunday morning, that the hæmorrhage had ceased probably upon the Saturday. In all the cases that I have seen of hæmatemesis, excepting those depending upon ulceration of the stomach, the blood vomited was only red at first, and at each succeeding vomiting became darker and darker, mixed with clots. This would lead to the supposition that the exudation of blood is comparatively soon stopped, and that the indications of treatment are early to attempt, by proper support, to prevent the patient from sinking. In the present case, to have by stimulants excited the stomach, might have been followed by a removal of the plug of lymph, and a return of active hæmorrhage. Dr Duncan pointed out to me as a defect in the treatment, the omission of injecting blood into the veins, and I am bound to state my belief that such treatment upon the Sunday might have saved the life of the patient.

A case of hæmatemesis, which has since fallen under my observation, leads me to think that rupture of some of the blood-vessels may more frequently be the source of hæmorrhage than is generally imagined. A woman, aged 60, attempted to lift a very heavy weight; she felt no immediate inconvenience, but during the day felt her stomach swell, and a degree of nausea and of faintness, which symptoms rather increased during the night. On the next day, when standing up, she was seized with vertigo, which was immediately followed by a vomiting of large quantities of clots of blood, which she compared to jelly. This continued for two days at intervals, with black pitchy stools and great prostration of strength. She is now slowly convalescing. Was this hæmorrhage a consequence of ruptured blood-vessel from the effort? Her bowels had for some time been very costive, and in consequence of accumulations in them, there was no doubt considerable passive congestion of the mucous membrane.

13. *Three cases of Perforation of the Stomach.* By Dr Carson.

An Irish labourer, a powerful athletic man, about 45, described by his wife as a hard worker and drinker. He had particularly indulged at the last Christmas, and since then has laboured under symptoms of dyspepsia, with pain in the stomach, occasional vomiting, and a sense of fulness after his meals, but was not prevented from working. He was seized upon Saturday morning suddenly at his work with intense pain in the abdominal region, and from the description, all the symptoms of acute peritoneal inflammation. These continued without intermission until Sunday evening, when he was brought into the Northern Hospital moribund, and in a short time, before I could see him, he died. From the note sent to me by the house surgeon, descriptive of the symptoms, I was immediately convinced that it was perforation of the stomach, of which it was found by examination to be a good specimen. The abdomen, on a *post mortem* examination, was found to be distended, and its parietes being opened, the peritoneal membrane lining them, as also that covering the intestines, were intensely inflamed. The small intestines, distended with flatus, and a yellow matter like thick oatmeal gruel, were loosely matted together by an incoherent lymph. There was a considerable quantity of a yellow fluid, in which were to be seen globules of oil (castor oil) floating in the abdominal cavity. The perforation was in the posterior wall of the stomach, about two and a half inches below the cardiac orifice. The hole in the peritonæum was about the size of a split pea. The mucous and muscular coats were removed as by a punch to the extent of half an inch in diameter. The other viscera, as far as they were examined, presented nothing remarkable, and the general appearance of the body denoted robust health.

Dr Carson presented at the same time the stomach of a servant woman, aged 35, a patient of Mr Cock's, in which a similar perforation had taken place, attended with all the symptoms of acute peritonitis, and in which precisely the same *post mortem* appearances presented themselves. The patient died within eighteen hours after the first seizure, which took place when lifting a heavy child into bed. She had never complained previously of pain or uneasiness at the stomach, and had always enjoyed pretty good health. She was to have been married a fortnight after the seizure.

Dr Carson related in connection with these two cases that of a lady whom he had attended with symptoms of severe chronic gastritis, accompanied with a defined pain at the pit of the stomach and vomiting after meals. When upon a visit to some friends in Manchester, she was seized with violent hæmatemesis, from which, after the loss of a great quantity of blood, she slowly recovered with complete freedom from her stomach symptoms. About six months after she was found to be affected with ovarian tumour, which rapidly increased, and after several ineffectual attempts to evacuate its contents by tapping, terminated her existence by suffocation in less than six months. The tumour upon examination was found to be multilocular. In one large cyst was found about twenty-five pints of gelatinous fluid, and there were about twenty other cysts from the size of a child's head to that of an orange, each one containing a substance of a different character and consistence, attached to the lining membrane of the large sac. The sac was connected by adhesions easily broken up with the peritonæum lining the anterior abdominal walls. The posterior wall of the stomach was found adhering to the under surface of the liver to the extent of a crown piece, and on opening the stomach there was found the cicatrix of a circular ulceration of an inch in diameter in the centre of the adhering part. The bottom of this cicatrix was a thin transparent membrane, through which was seen the substance of the liver, into which the ulceration, of which the cicatrix was the consequence, had eaten to the depth of a line. This had no doubt been the source of the hæmorrhage twelve months previous to the death of the patient.

14. *Case of Disease of the Kidneys associated with Hypertrophy of the Liver, recent specimens of which were placed before the Society.* By John Macnaught, M. D., Fellow of the Royal College of Physicians, Edinburgh.

A. B. about 38 years of age, of spare habit, but until within the last twelvemonths tolerably vigorous and active, had been regular in his diet, and generally temperate in the use of wine and other fermented liquors.

From an early period of life he had been subject to obtuse sense of weight and pain in the left side, extending downwards in the lumbar region, which had been referred to and treated as disease of the spleen. It never had exerted any effect on his general health, nor had it at any time interfered materially with professional pursuits. Upwards of two years ago, whilst sailing in a steam-vessel, and in his usual health, he felt as if something had burst in his left loin, and soon after, in the act of micturition, he observed the urine mixed with white opaque matter, which formed a deposit at the bottom of the vessel. He had at the time no difficulty in passing water, was a little sick and faint, but had no feeling approaching to rigor. Since then he has invariably voided with his urine purulent matter; which he supposes must have amounted to at least two ounces daily, making allowance for variation, being at one time much greater than at another. About ten years ago he underwent several operations for the cure of *fistulæ in perinæo*, the result of gonorrhœal inflammation. From that circumstance, and from observing that during the first efforts at vesical contraction there was always a discharge of apparently pure purulent matter previously to the flow of urine, I was induced carefully to examine the state of the urethra, prostate gland, and neck of the bladder, but no trace of disease was discoverable in any. There was considerable tenderness, but no perceptible fulness or inequality in the left lumbar region. Pressure, however, on this part, or frictions of the most gentle kind, seemed always to increase the purulent discharge. Lying on the left side had also the same effect.

In the lower part of the left side of the chest there was great dulness and altered respiration, indicating either condensation of the lung or the encroachment of some sub-diaphragmatic tumour. The liver was enormously increased in volume and in breadth. It extended into the left hypochondrium, and its edge was distinctly felt at the umbilicus. He was not aware until lately that there was anything wrong with his liver. He never experienced pain or tenderness, but for the last twelve months had found fault with his tailor repeatedly for making his clothes too tight, and is inclined to date the period of the hepatic growth from that time, as Dr Prout had carefully examined him just before, without making allusion to the disease of the liver. Dr Symonds of Bristol was the first who told him, about eight months ago, that his liver was diseased. Never had jaundice. Appetite good at present, and has been so throughout his illness. His skin had a husky withered appearance, and was without any sensible perspiration for a long while. He only came under my care about four months ago, and at that time it was too evident the disease was rapidly running its fatal course; he had a wasted look with frequent pulse, enlargement of the abdomen, and œdema of the feet and ankles. For a few weeks during the summer he seemed to rally, but the bowels became irregular. Constipation alternating with abundant serous diarrhœa mixed with blood.

Anasarca of the lower extremities and abdominal dropsy soon followed. The belly became so large and tense that I was induced to yield to his earnest solicitation, and perform the operation of paracentesis. About fifteen pints of turbid serum were drawn off, but the fluid reaccumulated to an equal extent in the course of three days. He was again tapped with temporary relief, but the diarrhœa returned, hectic fever increased, and he died about ten days after the second tapping with but little pain, and no impairment of the intellect.

Inspection of the body 26 hours after death. The body was greatly emaciated, and there was slight œdema of the legs, on which were a few petechiæ.

The abdomen was considerably enlarged, its parietes very much attenuated, and its cavity containing about twelve pints of slightly turbid serum with shreds of coagulable lymph. The peritonæum much congested and studded generally, but particularly in its pelvic portion, with small granules of lymph. The liver was enlarged to at least four times its natural size, filling the upper half of the abdomen. The inferior margin extending rather below the umbilicus, whilst its upper edge had encroached very much on the cavity of the chest, both in the right and left side, corresponding to the upper margin of the fourth rib. It weighed full twelve pounds.

On cutting into it it presented a smooth homogeneous red section, somewhat mottled, but without any appearance of lobules. Here and there were a few whitish yellow-looking spots, not unlike miliary tubercles, and most numerous at the surface. On examining the former section under the microscope no cells could be distinctly made out. Here and there they could be detected full of solid matter and considerably increased in size. None of them contained any globules. From a section of the whitish-yellow spots, however, the cells of the liver were immediately recognised, the quantity of fatty matter contained in these being much greater than usual. No granules of bile or deposits of fibrine in globules could be detected anywhere. It was slightly adherent to the diaphragm by recently deposited lymph. Capsule somewhat thickened, but easily separated without any part of its substance adhering. The gall-bladder was moderately distended with yellow bile.

The spleen was likewise considerably hypertrophied, being about three times its natural extent; its texture was somewhat softer than usual, and it seemed to occupy a position higher than natural, as if covered and pressed up by the liver.

The left kidney, which was also very much enlarged, had formed extensive adhesions with the surrounding organs. On looking into it a large quantity of purulent matter escaped to the extent of about six ounces, which had occupied large cavities, in each of which were found phosphatic calculi varying in size from a horse bean to a small walnut, and some of them assuming the shape of branched coral. There seemed little or none of healthy structure of the organ left.

The kidney on the right side was perceptibly enlarged; the cortical substance was of a grey appearance, and which had invaded, to a considerable extent, the tubular portion also. Stomach, intestines, and urinary bladder healthy, the latter appeared a little thickened, and had formed adhesions to the small intestines at its fundus.

Chest.—Its cavity was diminished full two-thirds the natural extent by the encroachment of the liver and spleen. There were about twelve ounces of turbid serum in either side. The lower lobes of both lungs were very much compressed, congested, and adherent to the upper and posterior part of the chest and diaphragm. They were free from tubercles; their apices, however, presented a marked appearance of cicatrices, having a puckered surface, and otherwise indicating the existence of some previous lesion. On being cut into at these points, white calcareous substances were noticed immediately beneath the surface about the size of small peas.

The heart was under the ordinary size but free from disease. The head was not examined.

Remarks.—From the history of this case it may be fairly inferred, that the primary disease was situated in the kidney; and it affords a well marked illustration that the worst consequences of long continued irritation from calculi in that organ may take place without any of those distressing symptoms which generally indicate their presence. Here the disease was evidently of long standing; but so obscure were the symptoms, and so little was the general health affected, that until the appearance of purulent urine, the kidneys were not even suspected as being implicated. The discharge of purulent matter by the urethra continued to the last, but for many months after its commencement, the patient was enabled, according to his own account, to attend to mercantile pursuits, and to travel on the continent with but little inconvenience, thus showing that inflammation, suppuration, and ulceration of this important gland may proceed slowly and to a considerable extent without any great amount of constitutional disturbance. The appetite was good, and the digestive functions continued active until within a short period before death. I ought to have mentioned that he had suffered from occasional attacks of hæmaturia, which, however, were readily controlled by the usual treatment. The urine from the time I saw him always gave the acid reaction, and in quantity was pretty natural until dropsy supervened.

The morbid change in the liver seems to be more nearly allied to the scrofulous than to the fatty degeneration, and I think may be ascribed to a modification of the former, and dependent most probably upon the same cause. Its progress throughout was not marked by any hepatic symptom further than that afforded by increased size. There was neither pain nor increased tenderness at any time. It commenced long subsequent to the original disease, and may be dated from a period when the vital powers were much weakened by the continued drain from the kidney. The heart's action was feeble, and may have given rise also to preternatural deposit in the liver, by producing congestion in the portal system.

13. *Case of Disease of the Hip-joint, terminating in Dropsy, with Albuminaria.* By T. INMAN, M. D.

John Williams, aged 29, pit-driver for twenty years, was admitted with disease of the hip-joint. He stated that three years ago he was bitten in the thigh by a dog so severely that he was confined to bed for two months. To this he attributes the origin of the affection of the hip-joint, as he was never able after the injury to walk without the aid of a stick. The man being very dull, no particulars could be obtained beyond the fact, that there had been suppuration for eighteen months. Soon after admission he directed my attention to his right arm and hand, which were swollen and œdematous. This led to an examination of the urine, which was found to be strongly albuminous, and of the loins, where there was considerable tenderness on pressure. On examining the former more minutely, it was found to be alkaline, and to contain very large quantities of the lithate of ammonia; in addition to this a number of microscopic shreds, (probably casts of the urinary tubes,) to which were adherent a few globules of fatty matter, and a number of scales of epithelium.

In a short time the dropsy extended to the whole body, and the discharge from the hip became entirely suspended. He was then transferred to my colleague, Dr Macrorie, under whose treatment he appeared to be improving rapidly, when he suddenly lay down and died.

Post mortem.—Head not allowed to be examined.

Kidneys large, white, streaked with radiating veins, granular when torn. The capsule was easily removed, and the pyramidal bodies appeared healthy. The specific gravity of the two was 1.038.

The lungs were œdematous, otherwise healthy. The heart, aorta, and valves, were likewise healthy. The liver appeared sound. The serous cavities were filled with limpid serum, but the membranes were free from inflammation.

On examining the kidney with the microscope the *tubuli urini-*

feri were found loaded with fat, which was contained in the cells of epithelium. The intertubular spaces were likewise found to contain abundance of oil globules of various sizes. No other abnormal appearance could be found.

The aorta was found to contain a quantity of fatty globules, though not in sufficient quantity to be recognised by the eye.

In connection with this case I would wish to make a few remarks upon a mode of investigation which has hitherto been passed over,—that of taking the specific gravity of the organs in health and in disease. Dr G. Johnson has called the attention of the profession to the large quantity of fat in the kidney in granular degeneration and consumption; Mr Bowman to the quantity existing in the liver in phthisis. Mr Gulliver has shown its existence in the atheromatous deposits in arteries, &c. All persons, however, do not possess the power of submitting the various tissues to microscopic examination, and cannot, with the ordinary modes of examination, ascertain the extent of adipose accumulations. It becomes, therefore, a matter of some interest to find out whether this may not be ascertained by physical examination. A short experience with the microscope shows that the means employed hitherto have failed to demonstrate anything under an excessive quantity. One plan seemed to promise largely, and though it cannot be employed as anything more than an adjuvant, may be considered a tolerably good one.

On examining the specific gravity of a healthy liver and kidney, I found that it varied, that of the two being rarely the same in the same individual; that of the former ranges from 1045 to 1055; that of the latter from 1042 to 1050. If we take one, however, in which fatty degeneration has ensued, we find a reduction in direct proportion to the extent of the deposit, descending occasionally below 1000, but usually ranging from 1025 to 1036.

This test, however, is liable to many fallacies, and taken alone can never prove the existence of an abnormal amount of fatty matter, as we have occasionally had reason to observe, that cachectic diseases of a chronic nature reduce the specific gravity to a considerable degree, although not a particle of extraneous or foreign matter can be seen.

A fallacy also may exist if due care is not taken to make all the observations under the same circumstances. Thus a liver and kidney were examined from a patient dead of diseased heart, accompanied by dropsy and great congestion of the venous system, and found to be of the specific gravity of 1067 and 1056. After three days' maceration in water, however, this fell to 1046 and 1029.

If, however, the same circumstances are always observed, the examination always made at the same time after death, and with due precaution as to other fallacies, the test will be found a very useful one. Our space will not permit us here to enter into the variety of pathological subjects upon which this might be made to bear, and the important deductions which may be made by it as to the reduced quantity of solid matter assimilated in prolonged disease, &c.

Another point connected with this case is of far greater interest, the evident connection between the disease of the hip and the granular degeneration of the kidney. It is well known that there is nothing uncommon in the transference of morbid action from one part to another, especially in diseases of a lowering or cachectic character. Scrofulous disease of the knee, for example, will be cured by the supervention of consumption, and the reduction of enlarged cervical glands may go on *pari passu* with the invasion and progress of disease of the knee, hip, or lungs.

It by no means follows, however, that the new affection is exactly similar to the old, though it may be closely allied to it. The physical and microscopical characters of *morbus coxarius* differ widely from those of granular kidney, and yet there can be no reasonable doubt that they may depend upon a common cause.

It happens, too, in many cases of phthisis, (perhaps three-fourths,) that the liver is fatty to a morbid extent, that in one-third the kidney is in the same condition, and that in all the aorta is so to a marked degree; and yet, as we have endeavoured to show elsewhere, there is a wide difference between the usual forms of tubercle and fat.

We have found, too, in the course of our examination, that this degeneration is not confined to the tubercular or scrofulous diathesis alone, but exists in all those cases where the vital power is reduced by prolonged disease. In cancerous affections, and in dysentery, for example, we have met with the aorta, kidneys, and liver as fatty as in phthisis, a circumstance to which Dr G. Budd prominently refers in his excellent treatise on the liver.

It is almost impossible to refrain from speculating upon the causes which determine this change, more especially when we find it is one so extensively prevalent.

It has been previously noticed, in cases of tubercular or scrofulous disease, that the matters deposited, though of three or four kinds in appearance, concur in being unorganizable, or at least of very low vitality; as is fat likewise, when it occurs in other parts than the tissue especially set apart for its secretion. It does not, therefore, require any length of argument to prove that the diseases in which these deposits take place have their origin in a depressed state of the organization.

The whole body, with very few exceptions, in health and disease, is composed of microscopic cells and their products, and is being constantly renovated and changed by the development of new ones, and the reconversion of the old, and it is in these minute bodies that all morbid changes must take place.

Analogy and experience alike have taught us, that a constant access of pure air and light is as essential to the formation of healthy cells as a supply of proper nourishment, and that before any one can be generated or even retain its form, there must be in operation a vital force. By this last is given the faculty of making inorganic molecules for a time part of the living body, and according as this force is powerful or otherwise, we have the cells and their products of greater or less vitality. If it be reduced below a certain standard, the natural structure is not formed, but a substance of a lower organic character, incapable of performing any function, and which, not being endowed with vitality, acts as any other foreign body would do.

That there is a great reduction in the vital force in diseases accompanied by cachexia there can be no doubt, as it makes itself apparent in every organ, and in the performance of every function.

This necessarily reacts upon the development of the various tissues, and gives rise to degenerate products.

The theory of normal and abnormal cell development, and its bearing upon pathology, is far too intricate to be discussed here. The study of these minute bodies, however, is gradually becoming more generally and widely known, and their importance more fully appreciated.

The greatest care must be taken that every person shall have all the hygienic conditions necessary for healthy development, the abstraction of any one being as much to be deprecated as the loss of more. None of the essentials of health can be dispensed with, without producing disease or a tendency thereto. The first care, therefore, of the physician will be to prevent this; but if, by any chance, a diseased condition should have been produced, his object must be to restore the system again to the best possible condition for the formation of healthy products. This must necessarily be a work of time: unhealthy cells cannot immediately produce healthy ones. The change must be gradual, and hence it is why all or nearly all constitutional complaints are so slow of cure. Diseased action once being set up, and new products formed, we have lost to a great extent our power over them, and must often be content (after having restored the hygienic conditions essential to health) to look on while nature cures or kills.

16. *Case of Poisoning by Arsenic, in which a whole family was affected.* Communicated by J. Hakes, Esq. Read (with Remarks) by R. H. Brett, Ph. D., F. L. S., Lecturer on Chemistry and Medical Jurisprudence at the Liverpool School of Medicine.

The family in which the fatal accident occurred consisted of six individuals and a maid servant; the mother, Mrs Gilton, three boys, and two girls. They all lived at home except the eldest son, who was away the greater part of the day. It appears that the manufacture of certain mineral colours was carried on at the premises, and that arsenic was employed as one of the ingredients in this manufacture. It further appears that there was a well in the colour-house where the operations were conducted, and that this well furnished water not only to the works but also by pipes communicated with the house, where the water was used for ordinary domestic purposes.* All the family partook of the water on the premises, as did also a cow belonging to them. The workmen employed in carrying on the business did not take their meals at the house, and therefore took little or none of the water.

Mr Bickersteth was first called to West Derby, where the family resided, on Monday, February 16, and found the mother in a dying state; unconscious, surface cold, the arms being in continued motion about her head. She died in a few hours, but before death consciousness returned for a short time. He was told that the youngest son had died a week previous, as it was supposed, from hydrocephalus. The symptoms, however, of both mother and child appeared to have been bilious vomiting and purging, with great thirst and head affections. On this visit the rest of the family looked rather ill, but did not make any complaint. Two days after, viz. February 18, Mr Bickersteth was again sent for. The eldest son and one of the daughters were then suffering from bilious vomiting and purging, with great thirst, not, however, accompanied by pain or tenderness of any part. The daughter felt some curious sensations in her lower limbs; in both the pulse was very quick.

Mr Bickersteth suspected that some poisonous substance had been taken, and finding the water which they used came from a well in the colour-house, he thought it probable that arsenic was present; he prohibited the use of the water, and took samples away, which were subsequently delivered to me for analysis by the brother of the deceased lady. Upon analysis the water was found to contain arsenious acid or white arsenic in the proportion of four-tenths of a grain to the imperial pint.

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* A short time previous to the first person being taken ill, a quantity of arsenic had been thrown down a drain, which was subsequently found obstructed, and it is probable that rain water percolating through this had carried a portion into the well.

The following day, February 19, the eldest son was better and so was his uncle, who had only been there one day, and was seized after taking tea in the house. The two daughters and the youngest son were now suffering. The vomiting had ceased in the one first attacked under the administration of prussic acid, and had given way to nervous symptoms. There was great restlessness, indisposition to answer questions, numbness of the lower extremities, whilst the arms were in continual motion in the air, as if picking at something above the head; the skin was hot but not dry, and did not exhibit any discoloration; the tongue was dry and brown, and there was great thirst; the throat seemed dry, and the breathing was quick and audible; the pulse was very quick, 125 or 130, but not hard; the heart acted forcibly, but without friction, sound, or murmur. The girls did not complain of pain on pressing the abdomen, but the youngest son slightly moved when this was done, though he said it gave him no pain. They had all uncomfortable feelings, which they could not describe, but were at this time conscious.

On the following day, the 20th, they were much worse. The breathing and pulse very rapid; the tongue and lips dry, cracked, and covered with blood and sordes. The eldest girl was unconscious, throwing her arms, legs, and body about in every manner. The other two exhibited the same symptoms as yesterday, only in an increased degree, and consciousness was fast departing. (There had been no purging or vomiting in the girls since Wednesday the 18th.)

On Saturday the 21st the youngest boy died at 4 P. M.; the peculiar movement of the arms continuing up to the time of his death. Between that period and Sunday night the two girls died. The servant early at this period was taken ill and went home; at a later time the cow was affected. The workmen, with one exception, were not sufferers. A short time after the servant was admitted into the fever hospital with symptoms purely nervous, and after a few days was discharged cured.

Post mortem examination of the youngest son 24 hours after death.

There were no peculiar appearances noticed on the skin, and the muscles of the trunk were of their usual colour.

The lungs were adherent on both sides by old adhesions posteriorly; both were congested with bloody serum, especially the right. The bronchi were red and injected, and covered with red mucus.

The heart and pericardium were both healthy looking; and the blood in the body was dark and fluid generally, though there were coagula in the heart.

The trachea and epiglottis presented marks of inflammation.

The liver was slightly enlarged, presented exteriorly many yellowish-green patches, and was internally of an uniform slate or ash colour. Its consistence was normal; and the bile in the gall bladder was copious and dark.

The œsophagus was healthy-looking. The stomach was contracted, contained some greenish fluid and mucus, but, with the exception of punctiform and ramified redness at the splenic and pyloric ends, presented no unusual appearance. The rugæ were red and vascular; the mucous membrane was not softened or ulcerated.

The commencement of the duodenum was red like the stomach. A few patches of redness existed in the jejunum, and the lower part of the ileum was discoloured for about 12 inches, where the solitary glands appeared unusually large and numerous.

The cœcum was dark-coloured and congested. The rectum and colon were also discoloured here and there, but no ulceration or softening had occurred. The intestines contained a large amount of fæces. Spleen and kidneys were healthy.

Head.—Sinuses and veins congested; about a table-spoonful of serum existed at the base of the brain, and the same quantity in the ventricles; no softening or formation of false membrane was observed.

Remarks on the Fatal Cases of Poisoning by Arsenic, by R. H. Brett, Ph. D., F. L. S., Lecturer on Chemistry and Medical Jurisprudence at the Liverpool School of Medicine.

The above recited cases of arsenical poisoning are interesting in a toxicological point of view. They are clearly cases in which the poison is completely in solution, when it always exerts a more powerful influence than in lumps or in powder. With regard to the quantity taken in any one of the cases, only approximative results can of course be obtained. It would appear from a statement made to me by the brother of the deceased Mrs Gilton, that she was suffering about a week before her death; and as intense thirst was a prominent symptom with her, she probably did not consume less than from four to six pints of water during the 24 hours in different ways. Taking, therefore, four-tenths of a grain of arsenic in the pint as the average quantity, this lady in all probability took not less than two grains of the poison in a day, and in all perhaps not less than fourteen grains. It is not easy to understand how the other members of the family failed to be affected earlier than they appear to have been, considering that they must have been using the water before and during the illness of the mother, and yet no marked symptoms made their appearance until two or three days after her death. Had the mineral been accumulating in the system for some days before virulent symptoms were ushered in? Such could not, however, have been the case with respect to the lady's brother, for he had only been in the house one day, and was seized shortly after taking tea. It does not appear, however, that his symptoms were of a very formidable nature, for he speedily recovered. Moreover, the use of the water was entirely prohibited on the 10th February, two days after the mother's death; nevertheless the two girls and the youngest boy died; either therefore fatal results occurred in their case from the arsenic received in the water during two days, or they must have been swallowing the poison for some time previously without manifest symptoms. It is quite impossible, therefore, to say how much altogether of this substance they took before the use of the water was discontinued.

With regard to the symptoms recognised in these cases, they were rather of a nervous kind, than referable to irritation or inflammation either local or general. It is true, that in all the cases there was vomiting, purging, and great thirst, with a good deal of arterial action, but then there was almost entire absence of pain. The symptoms which appeared towards the close of life were clearly referable to the brain and nervous system. It might have been expected, considering that the patient survived some days, that more marked symptoms of irritation of the stomach and intestines would have been present, together with pain on pressure; nor do the *post mortem* appearances in the only case examined lead to the conclusion that active inflammation was a prominent symptom. The nervous symptoms were like those which have in several instances been observed in arsenical poisoning, as supervening upon symptoms of high irritation, with pain in stomach and bowels. The above cases also differ from those on record, from the fact, that there was, so to speak, a constant repetition of the poison, until the state of the water was suspected; it is impossible to say how many times during the day and night small quantities were taken into the stomach in the various forms in which water might be used. No doubt, much of the poison was rejected in consequence of the vomiting, which appeared to be a prominent symptom in all the cases; but still, as the mineral was completely in solution, it is highly probable that much more rapid absorption of it into the system took place than would have been the case had it been taken as a powder, or merely suspended in a liquid. It perhaps will not be unreasonable to attribute the absence of marked local irritation to the rapidity of absorption, by which the system generally became impregnated; and probably the quantity at any time present in the stomach was extremely small, and in consequence of the repeated vomiting not detained there for any considerable time. That there was a certain amount, however, of irritation, not to say inflammation of the stomach and bowels, the earlier symptoms in these cases certainly seem to indicate, and the nervous symptoms which followed were pretty well marked. Upon the whole, therefore, these lamentable cases may perhaps be classified with those in which symptoms of irritation, by no means well marked, were first observed, followed by nervous symptoms, such as have been frequently before noticed in cases of poisoning by arsenic, where circumstances favourable to its absorption existed.

Art. II.—*Observations on the plan of Treatment of Cases of Placenta Prævia proposed by Dr J. Y. Simpson, Professor of Midwifery in the University of Edinburgh, with a Statistical Report of such cases as have occurred in the practice of* JAMES RUSSELL, Esq., Fellow of the Royal College of Surgeons of England. Birmingham, April 1846.

THE great mortality said to attend upon cases of *placenta prævia* in the memoir upon that subject, published by Dr Simpson in the London and Edinburgh Journal of Medical Science for March 1845, to which my attention has lately been directed, is so contrary to my own experience, and so likely to excite needless alarm in the minds of young practitioners, that I am induced to beg a few pages of your journal to detail the result of my own practice in the management of those cases, extending, as it has done, over a period of nearly forty years.* In arranging my cases for this purpose, I have divided them into two classes; those which occurred in my private practice, and those to which I have been called in consultation with others, and which I therefore call consultation cases. It is from the first class alone, in my opinion, that any useful statistical deductions can be made as to the result of any particular plan of treatment. In the second class, the fatal cause has too often been allowed to proceed unchecked until the interference of art is unavailing. It is therefore unjust to test, as Dr Simpson appears to me to have done, the merits of any plan of treatment by the results of such cases.

The success which has attended the treatment of the cases in my private practice, I attribute entirely to my having rigidly acted upon the rule which I received early in my professional career, and for which I have always considered myself indebted to the late Dr Rigby of Norwich, viz. in cases of uterine hæmorrhage caused by the attachment of the placenta over the mouth of the womb, always to deliver as soon as the state of the os will permit you to pass your hand into the uterus. I think the perusal of the following cases will justify the fitness of this rule for general adoption. Where it has failed with me in saving life, its failure has generally been occasioned by delivery having been too long deferred. With this conviction on my mind of the great value of the rule in practice, I look with regret upon the announcement of any project which has a tendency to interfere with a line of conduct in the treatment of these appalling cases, which I believe to be safe, decisive, and practicable. It has not escaped my notice that Dr Simpson proposes his plan of treatment for those cases which do not admit of the introduction of the hand into the uterus. I respectfully submit that he should have confined his observations to such cases; and not have deduced his reason for advocating a new line of treatment from the fatality of those cases in which death has ensued, in consequence of the delay which has occurred in putting in practice the prescribed mode of treatment. It would be indecorous in me to assert that cases never occur in which it is impossible to introduce the hand into the gravid uterus when arrived at the seventh month of pregnancy, when Dr R. Lee, Dr Simpson and others mention such cases. But with every respect for such high authorities, I venture to say that they are of very rare occurrence. I have never met with a case in which it has been necessary to deliver in the seventh month of pregnancy, nor indeed, I may add, even in the sixth month, in which I have not been able to introduce my hand at some period of the labour. In confirmation of this statement I refer to cases number 45, 50, 304, 445, 1469, 1810, 2023, 2520.

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* To obviate any apparent discrepancy between the above assertion and the dates of the cases, I beg to state, that my health compelled me to relinquish in 1835 my connection with public institutions. A reference to the dates of the cases in my private practice will, however, bear out the correctness of the above statement.

The few observations which I have made upon Dr Simpson's proposed plan of treatment in cases of *placenta prævia* have been dictated solely by a sense of duty. Several fatal instances have come to my knowledge in which the death of the mother has been attributable entirely to a want of firmness and courage on the part of the attending practitioner; I therefore regard with a jealous eye any proposal which appears to sanction in the minds of timid and young practitioners, an undecided and wavering practice in the treatment of cases in which safety entirely depends upon a prompt and decided line of conduct.

In arranging my cases I have inserted the numbers as they occur in my case book. No instance of fatality to the mother occurs in my *private practice;* in my *consultation practice* the cases fatal to the mother are Nos. 213, 290, 348, 445, 970.

Out of 2304 deliveries in *private* practice *placenta prævia* has occurred to me 7 times, making an average of 1 in 328 deliveries. The lives of all the mothers and six of the children were saved: of the seventh child I have not preserved any record. In 230 cases in *consultation* 29 have been cases of *placenta prævia*. Of this number 5 mothers died, while 24 were saved, making an average of nearly 1 death in 6 deliveries. If the number of private cases be added to the consultation cases the average of death is reduced to 1 in 7. Of the children in the second class of cases 18 were born alive; of 7 I have no record; and 4 are recorded as born dead.

Out of 2534 deliveries, *i. e.*, the private and consultation cases added together, it has only once occurred to me to meet with the placenta completely expelled from the uterus before the child, viz. in No. 47; in two instances, viz. 797 and 813, the placenta was in part protruded into the vagina, the child in each case was turned, and in each case the life of the mother was saved; in 797 the child was dead; in 813 there is no record of the fate of the child.

Cases which occurred in private practice. 72, 80, 324, 1810, 1997, 2023, 2500.—72. A young married woman, 26 years of age, pregnant with her first child, engaged me to attend her, having had during her pregnancy several attacks of flooding. I was called to her on February 1st 1814: she had flooded very profusely. I found the *os uteri* dilated to an inch and a half diameter, and covered by the placenta: she was immediately delivered, and both lives were saved.

80. Mrs W. had been flooding at intervals during the previous fortnight. I was not called to her until the evening of March 24, 1814. The *os uteri* was sufficiently dilated for me to ascertain that the placenta presented. She was exceedingly reduced in strength; and fearing the consequence to her of further loss of blood, I immediately delivered her. The child was born alive, and the mother speedily recovered.

324. September 26, 1819. Mrs Y. was awakened out of her sleep by slight pain, accompanied by profuse hæmorrhage, and immediately sent for me. I learnt that she had been flooding at intervals the three preceding days. She had lost a great deal of blood, and the hæmorrhage was going on. She was a corpulent person, and I had great difficulty in reaching the *os uteri*, which I found sufficiently dilated for me to assure myself that the placenta presented. I had some difficulty in passing my hand into the uterus, but having effected this I readily completed the delivery: the mother speedily recovered. I have no record of the state of the child.

1810. August 9, 1833. A delicate lady, the mother of seven children, was seized early in the morning of the 6th with slight pains, shivering, and hæmorrhage. I found her greatly alarmed, but the pain and hæmorrhage had ceased. The *os uteri* was situated very high, and was quite closed. Coolness and perfect rest were enjoined, and she lay in bed the remainder of that day, and also of the following. When I called on the 8th she was down stairs lying upon a sofa: she informed me that her water had broken, and she had neither pain nor discharge. On the 9th flooding returned with great profuseness, the *os uteri* was much lower down, and dilated sufficiently for me to make out that it was nearly covered by the placenta. As she was greatly reduced, and as hæmorrhage had returned, notwithstanding the partial discharge of the *liquor amnii*, I did not hesitate about delivering her. The child was born alive, the mother was feverish after her confinement, but has since had three children.

1997. July 14, 1835. The weather was intensely hot. This lady, in her fifth pregnancy, was suddenly seized with flooding, which was continuing when I arrived. I found her faint, her countenance bleached, and her pulse small and quick. I found the *os uteri* dilated to an inch and a half diameter, and covered by the placenta. I availed myself of the opinion of my friend Mr Vickers, and we both agreed she should be instantly delivered. The mother recovered the shock of the delivery, and the child was born alive.

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2023. July 24, 1833. The wife of a respectable publican was seized, while serving her customers, with sudden and profuse hæmorrhage. She was seven months advanced in pregnancy of her seventh child. When I saw her, the flooding had ceased. She was kept quiet in bed for three days, at the end of which time she dressed herself and came down stairs; the consequence of which was the immediate return of the hæmorrhage. On examination, I found the *os uteri* quite closed. I left her cheerful and comfortable, and was called to her again early the following morning. She had been flooding during the night, and having fainted, her attendants became alarmed. The *os uteri* was dilated to the size of a shilling, and I could feel the placenta attached over it. I accomplished the delivery with much more ease than I anticipated. The child lived some days, and I have lately delivered the mother of her eleventh child.

2500. An extremely delicate lady, the mother of several children, consulted me during her pregnancy, in consequence of feeling great weakness and exhaustion. She had had two seizures of flooding before I saw her. On the morning of June 15, 1845, she was taken with slight labour pains, which were attended by discharge. She was in her eighth month of pregnancy. When I arrived, the flooding had ceased; and having placed her in bed, I left her, and saw her several times during the day. At nine o'clock in the evening I was again summoned to her; there were both pain and discharge. The *os uteri* was situated very high, and dilated just enough to pass the finger through and to feel the rough surface of the placenta. In an hour's time the *os uteri* became more dilated, and I could then make out that it was only partially covered by the placenta. I attempted to rupture the membranes, but the hæmorrhage returning, and my patient becoming faint, I determined upon immediate delivery; and having obtained the co-operation of my friend Mr Wickenden, I accomplished the delivery without difficulty. Both mother and child are living at the present time. Immediately after her delivery she was carefully bandaged, but soon became faint, with a sensation of dying; and this condition supervened upon the slightest escape of discharge. There was, however, a good pulse and a contracted uterus; but the remainder of the night was spent in a state of great anxiety and exertion to all around her. With the view of checking the discharge, a few syringes full of cold water were thrown into the uterus: this gave great pain: it had the effect of checking the discharge at the time; but when reaction took place, the hæmorrhage returned, so that I think it a very doubtful remedy. I was able to leave her at six o'clock in the morning, and she gradually recovered, until the sixth day after delivery, when the hæmorrhage again returned, and continued at intervals for the ten following days, during which time my patient was a source of the greatest anxiety I found the most effectual means of checking the tendency to hæmorrhage was the application of bladders filled with ice to the pubic region. These were continued until the tendency to hæmorrhage had ceased. The other remedies applied were such as would occur to the minds of every intelligent practitioner. Tannin was injected, but without any marked benefit.

Cases which occurred in consultation practice. 46, 45, 47, 50, 51, 28, 124, 213, 269, 282, 290, 304, 348, 438, 445, 486, 623, 670, 695, 797, 813, 960, 970, 971, 1001, 1164, 1469, 1825, 1978.

46. July 10, 1809. This poor woman had been flooding very profusely. The *os uteri* was dilated to the size of half-a-crown, and entirely covered by the placenta. She was immediately delivered, and both lives were saved.

45. June 10, 1811. In this case there had been very profuse hæmorrhage. The *os uteri* was partially covered by the placenta, and was dilated to nearly the size of a crown. The patient was exceedingly exhausted, and I expected to deliver her with ease. In this expectation I was disappointed. The *os uteri* would not yield to the dilating power of my hand, and I was obliged to desist. I met with a foot near to the *os uteri*, which I brought down, and then gave my patient three grains of opium. In about an

hour after, she complained of sickness, and had a slight pain. The contractile power of the *os uteri* gave way, and I was able to complete the delivery with great ease. The mother recovered. I have no record of the child.

47. July 1811. In this case there had been considerable hæmorrhage. I found the entire placenta in the vagina. I drew away the placenta, which was immediately followed by a seven months' dead child. The woman slowly recovered.

50. July 6, 1811. This poor woman was more than eight months advanced in pregnancy. She had had profuse hæmorrhage before I saw her, which was still going on. The *os uteri* was high up and very little dilated. Her strength and spirits were good. I plugged the vagina with a handkerchief, and saw her again in an hour. I then found there had been slight pains, with considerable oozing by the sides of the plug; on removing which, I found the *os uteri* dilated to the size of a shilling, and covered by the placenta. I proceeded to deliver her, and both mother and child were saved.

51. December 13, 1811. This woman had been attended by an ignorant man, and had been flooding for ten days, when my friend, the late Mr Baynham, was called to her, who sent for me. She was in a state of great exhaustion; the *os uteri* was well dilated, and was covered by the placenta. I immediately delivered her. The child was born alive, and the mother slowly recovered.

28. May 21, 1813. I found this patient in a state of great exhaustion from flooding. The *os uteri* was dilated to the size of half-a-crown, and covered by the placenta. She was immediately delivered, and both lives were saved.

124. This patient had been flooding for several days previous to my seeing her; the *liquor amnii* had been discharged before I saw her. I found the *os uteri* well dilated and covered by the placenta; the hæmorrhage continued. I proceeded to deliver her. She bore the delivery very well, and recovered from the shock of the delivery, but died some days after from neglect and poverty. I have no record of the state of the child.

213. May 24, 1816. This poor woman was in her seventh month, and had been flooding since the 18th. Her midwife was greatly alarmed at her state, and thought she would have died before I could reach her. I found the vagina filled with coagula; the *os uteri* dilated to the size of half-a-crown, and covered by placenta. Half a pint of brandy was given her, and I immediately delivered her. She died a few hours after her delivery; the child survived.

269. February 1, 1817. I merely record that this patient was immediately delivered, and did well.

282. April 9, 1817. This patient had lost a considerable

quantity of blood when I saw her. The *os uteri* was sufficiently dilated to ascertain that the placenta presented. I proceeded to deliver her, and both mother and child did well.

290. May 14, 1817. A poor girl was nearly at her full time of gestation with an illegitimate child. She was brought home in a coach from her work in consequence of hæmorrhage, and I learned that she had several attacks during her pregnancy. When I saw her the flooding had ceased. I gave full directions as to her management, and said that I was to be called if the hæmorrhage returned. I was summoned to her at four o'clock the following morning, as the flooding had returned during the night. She was greatly exhausted, her pulse could not be felt, and her skin was blanched and cold. The placenta was attached over the *os uteri*, which was dilated sufficiently for me to pass through my three fingers. A glass of brandy was given to her, and I immediately delivered her, but she died in two hours afterwards. The child was dead.

304. July 5, 1817. This patient was seized with flooding at four o'clock in the morning. She was visited at that time by an eminent accoucheur, who had ascertained that it was a case of placenta presentation. He attempted to deliver the woman, but not being able to pass his hand through the *os uteri*, he gave her one hundred drops of laudanum. I was called to her in his absence at eleven o'clock. She was exceedingly exhausted, and I delivered her without difficulty. Both the mother and child were saved.

348. January 25, 1818. An instance of culpable neglect. This poor woman had been flooding the whole of the night, and the midwife had not sent for assistance till mid-day. I found the poor creature in a moribund state. The *os uteri* was dilated to the size of half-a-crown, and covered by the placenta. After giving her half a pint of brandy, I delivered her, but she died immediately afterwards. I remark that I had more difficulty in dilating the *os uteri* than I anticipated, but that when the waters escaped the mouth of the womb immediately relaxed.

438. October 27, 1818. Was attended by my brother. Our patient was in her seventh month of pregnancy, and had been seized with flooding, which had ceased on my arrival. I found the *os uteri* dilated to the size of an inch and a half in diameter, and partially covered by the placenta. The membranes were ruptured, and we left her. In the middle of the night we were again called to her; labour pains had come on, the head was bearing upon the perineum, and in a short time she was safely delivered.

445. December 1, 1818. This patient was seized during the night with profuse hæmorrhage, which had ceased before I arrived.

I found the *os uteri* so closed, that I could not enter it. I was not called to her again till the middle of the day, when I learned that she had had slight pains, and with each pain discharge of blood. I found the *os uteri* much lower down, and so far dilated that I could ascertain it was completely covered by the placenta. I had little difficulty in delivering her; and after bandaging her, and making her comfortable, I left her in a very satisfactory state. In less than two hours I was again summoned to her; the hæmorrhage had returned, and she died as I entered the room. The child lived.

486. March 15, 1819. This patient had flooded several times during her pregnancy. She had had slight pains during the day, and each pain was attended by discharge. Her midwife being called to her, immediately sent for me. I found the *os uteri* dilated to the size of half-a-crown, and in part covered by the placenta. As she was much exhausted from the loss of blood I delivered her, and both lives were saved.

623. March 14, 1820. Was in the eighth month of pregnancy. The circumstances of this case exactly resembled those of 486, except that the entire *os* was covered by the placenta. She was delivered, and both lives were saved.

670. I was called to this poor woman July 16, 1820. She was between the seventh and eighth month advanced in pregnancy, and had been taken with violent hæmorrhage. The discharge had ceased when I saw her, and after a few days of perfect rest she was able to resume her domestic duties. On August 20th I was again summoned to her; the hæmorrhage had returned with great violence; the *os uteri* was dilated to the size of one inch and a-half diameter, and covered by the placenta. I had some difficulty in getting my hand into the uterus. Both lives were saved.

695. September 15, 1820. Was attended by two medical friends. She had been flooding two days, and was greatly exhausted. The *os uteri* was well dilated and covered by the placenta. I had little difficulty in delivering her, and she speedily recovered. I have no record of the child.

797. March 26, 1821. This poor woman, the mother of nine children, had been flooding very profusely. I found nearly the whole of the placenta protruded into the vagina. I immediately delivered her. The child was dead; the mother recovered.

813. April 18, 1821. I was sent for by two medical friends to a patient who was very much exhausted in consequence of loss of blood. When I saw her she had fainted. On examination I found a portion of the placenta protruding through the *os uteri*. I advised immediate delivery. The mother recovered; there is no record of the child.

960. April 11, 1822. A poor debilitated woman was delivered at eight o'clock in the evening of a female child. Severe hæmorrhage following the birth of the child, I was sent for. I found the vagina filled with coagulated blood, and the *os uteri* covered in part by the placenta and membranes of a second child. Having given her a glass of brandy with water, I proceeded to deliver her. She slowly, but perfectly recovered. The child lived.

970. April 26, 1822. This poor woman had been flooding the whole of the day. I was not called to her before the evening. I found the *os uteri* dilated to the size of an inch and a half diameter, and covered by the placenta. In making my examination I separated a portion of the placenta from the uterus, which was immediately followed by a gush of blood. Another gush succeeded whilst I was preparing to deliver her. She was delivered with great ease, but she could not rally from the effects of the quantity of blood she had lost, and died in an hour after her delivery; the child lived. I remark here the ease with which the *os uteri* dilated when the waters passed away. This poor woman was greatly reduced by poverty and hard labour.

971. June 1, 1822. The mistress of the last poor woman was pregnant with her first child, and was seized with flooding under exactly similar circumstances as her late work-woman. I found her very faint and greatly exhausted. The *os uteri* was dilated to the size of an inch and a half diameter: it was thin. I immediately proceeded to deliver her, and both lives were saved.

1001. August 4, 1822. I accompanied my pupil, the late Mr Parsons, to this woman, 26 years of age, whom I found had had attacks of flooding for two days previous. She was faint; her pulse was quick. I found the *os uteri* dilated to the size of an inch and a half diameter; it was covered by the placenta. I was alarmed at her state, and after giving her some brandy, proceeded to deliver her. After the birth of two children, the placentæ were expelled separate from each other by the contractile power of the uterus. Both mother and children did well.

1164. March 18, 1824. Was in the eighth month of pregnancy. She had been flooding profusely for two hours. When I saw her the *os uteri* was dilated to two inches diameter, and was covered by the placenta. I had no difficulty in delivering her. The child made some efforts to live, but we were not able to reanimate it. The mother recovered.

1469. January 29, 1829. I was called to this patient early in the morning. She had been flooding very profusely. On examination I found the *os uteri* situated very high and quite closed. There was hæmorrhage, and after giving the midwife instructions, I left, and was called to her again the following morning. She had been taken with slight labour pains, and each pain was attended by discharge. I found the *os uteri* sufficiently dilated to admit of my ascertaining that the placenta presented. As she was much reduced, I immediately proceeded to deliver her, and both lives were saved.

1825. October 19, 1833. This poor woman had flooded several times during her pregnancy. She had had eleven children, and was at this time greatly reduced by hæmorrhage and poverty. I found the *os uteri* dilated to two inches diameter, very flaccid, and covered by the placenta. I had no difficulty in delivering her. She rallied from the delivery, but died in a few days after of gastric fever. I have no record of the child.

1978. December 29, 1835. This was a case of placenta presentation. I only record that she was immediately delivered, and both lives were saved.

ART. III.—*On the Period of Puberty in Hindu Women.* By JOHN ROBERTON, Esq., Surgeon, Manchester.

IN some previous papers tables have been given to show the age of puberty in Bengal and in the Deccan, the number of instances amounting to 310; and with reference to the ages of mothers at the birth of a first child there have been 163 instances: the whole collected for me by medical practitioners, two of them Hindu and three European.

Within the last month I have been most kindly favoured by A. H. Leith, Esq., of the Bombay Medical Service, with an additional table from Bombay supplying, as to the period of puberty, 230 examples; and with respect to the ages of mothers at a first birth, 102 examples. These fresh data, which come from a quarter of Hindustan different from any of the preceding, swell the total number of examples as to puberty to 540, and the number in respect to the ages of mothers at a first birth to 265. Our information, therefore, concerning both these points may now be regarded as tolerably complete; particularly as the facts, obtained from places widely apart from each other and parties having no connection or intercourse, agree in a remarkable manner; presenting physiological phenomena of a character *peculiar*, so far as is yet known, to the people of India: at all events differing, by well marked features, from what we find in Europe. The results upon the whole would seem to open a field for further investigation, not merely because they differ in some respects from what I had anticipated, but differ also no less, as we shall see, from what European physiologists had all along conceived of them.

The care with which the Bombay table has been compiled will be best shown by the letter of its author. None of the returns I have hitherto received have come before me with claims to accuracy, so far as I can judge, superior to the present.

Bombay, January 24, 1846.

Dear Sir,—Being in a situation giving me facilities of obtaining information regarding the age at which females arrive at puberty in this climate, I lately collected a number of cases with a view to determine that period, and also the earliest period of susceptibility of impregnation, and I beg to forward a copy of them to you. These cases were collected with care through three native medical practitioners, and a *Joshee* or astrologer, whose livelihood is gained by preparing nativities or *junum putree.* The ages of the individuals were verified by my agents by reference to the birth paper or *junum putree* of each, and the date of the birth of the first child was verified by reference to its birth paper. The periods of the first appearance of the catamenia were necessarily obtained from the reports of the individuals and their relatives, but as it is an event generally remarked by a feast, and as care was taken in the investigation of the cases, I believe that the periods stated are close approximations to the truth—indeed in many of the cases the time was noted to a day. The present ages of the individuals were furnished to me, but as the introduction of them into the table would greatly increase the number of figures, with but little additional benefit, I have omitted them.

The cases were taken only from those castes of Hindus that keep birth papers, and from the *Parsees* or fire-worshippers, who have adopted this among other customs from their Hindu compatriots. Among all these betrothal takes place in childhood, and the husband and wife live together from the appearance of puberty. I remain, dear Sir, yours faithfully,

A. H. Leith,
Bombay Medical Service.

Cases collected at Bombay in 1845.

No.	Caste.	Age when catamenia commenced.			Age when first child was born.			No.	Caste.	Age when catamenia commenced.			Age when first child was born.		
		Y.	M	D.	Y.	M	D.			Y.	M	D.	Y.	M	D.
1	Brahmun,	12	...	...	...	...	...	10	Brahmun	13	3	...	...	...	...
2	...	12	...	...	...	...	...	11	...	13	6	5	14	2	19
3	...	13	...	...	...	...	...	12	.	12	7	...	...	...	...
4	...	13	8	...	...	...	...	13	...	13	...	...	15	...	28
5	...	14	3	...	...	...	..	14	...	14	1	...	16	...	17
6	...	11	4	17	..	...	...	15	...	14	...	...	15	...	21
7	...	11	1	7	16	3	22	16	...	12	6	...	13	6	..
8	...	11	9	...	17	2	15	17	...	13	2	...	...	...	...
9	...	12	..	...	19	3	...	18	...	12	5	:	13	9	...

No.	Caste.	Age when catamenia commenced.			Age when first child was born.		
		Y.	M	D.	Y	M	D.
19	...	14	...	...	...	...	...
20	...	16	...		17	3	...
21	...	13	2	...	...	...	...
22	...	15	1	...	16	9	...
23	...	18	...	...	..	...	...
24	Purbhoo,	13	6	...	...	...	...
25	..	13	...	27	...	...	...
26	...	12	2	...	...	...	...
27	...	12	...	22	13	4	27
28	...	11	6	...	12	6	...
29	...	11	...	...	...	...	...
30	...	14	...	11	..	...	...
31	...	13	2	7	...	..	...
32	...	13	.	...	...	...	...
33	...	14	7	...	16	2	9
34	...	12	...	...	..	...	...
35	...	14	...	...	...	...	...
36	...	11	5	..	..	...	...
37	...	13	...	...	...	.	...
38	...	15	...	...	...	...	...
39	...	14	...	...	15	...	...
40	...	12	...	...	...	. .	...
41	...	15	...	..	17	3	...
42	...	13	6	...	...	...	...
43	...	12	3	...	14	...	...
44	...	16	2	...	...	...	...
45	...	12	2	. .	14	8	...
46	...	17	...	...	...	...	...
47	Bunya,	11	...	...	...	...	...
48	...	12	...	...	..	...	...
49	...	14	...	...	..	...	...
50	...	13	...	...	...	...	...
51	...	16	.	...	...	...	...
52	...	12	9	.	...	...	...
53	...	11	...	9	12	6	...
54	..	11	3	...	..	...	...
55	...	12	...	...	...	..	...
56	...	12	...	...	13	9	...
57	...	14	11	...	15	7	...
58	...	13	2	...	...	...	...
59	...	15	1	9	17	1	...
60	...	17	...	3	...	...	...
61	...	14	...	...	15	...	...
62	...	12	4	...	16	...	...
63	..	13	...	...	...	...	...
64	...	12	..	...	...	...	...
65	...	15	...	. .	20	..	...
66	...	12	1	...	...	..	..
67	...	11	7	...	15	...	...
68	..	13	10	...	...	..	...
69	...	15	4	...	...	...	...
70	...	10	...	..	13	...	...
71	...	13	...	...	15	...	...
72	..	12	..	...	15	...	.
73	...	11	...	..	...	...	...
74	...	11	...	...	22	...	...
75	...	10	...	...	...	...	...
76	...	16	...	...	22	..	...
77	Bunya	13	...	..	...	...	..
78	...	12	..	...	13	...	...
79	..	13	...	...	...	...	...
80	Bhatia	13	...	...	15	1	...
81	...	12	3	...	...	. .	...
82	...	11	6	...	...	...	...
83	...	12	...	8	...	...	...
84	...	12	..	...	13	7	...
85	...	13	9	...	...	...	...
86	...	14	...	...	15	9	...
87	. .	12	...	...	...	...	...
88	...	13	2	5	...	...	...
89	...	12	...	..	..	...	...
90	Sonar,	11	...	9	...	...	...
91	...	11	9	...	14	7	...
92	...	14	..	...	18	4	...
93	...	17	...	...	21	8	...
94	...	15	3	...	...	.	...
95	...	13	...	...	...	...	...
96	...	13	...	...	...	...	...
97	...	11	..	17	..	...	...
98	...	13	1	...	15	6	...
99	...	12	3	...	...	...	. .
100	...	12	...	...	...	...	...
101	...	11	4	...		...	...
102	...	13	...	...	...	...	
103	...	13	6	...	...	...	...
104	...	15	...	...	...	...	...
105	...	13	...	...	14	6	...
106	...	12	2	..	...	...	.
107	...	15	...	...	16	7	...
108	...	14	...	...	..	...	...
109	...	12	...		14	...	...
110	...	11	8	...	...	...	..
111	...	15		..	20	.	...
112	...	13		15	..	...	...
113	Kansur,	11	9	25	15	9	...
114	.	11	...	...	..	...	...
115		14	...	...	...	...	...
116	...	13	...	...	16	2	. .
117	...	12	..	...	13	6	...
118	..	13	2	..	...	...	...
119	...	12	1	9	...	...	..
120	...	12	...	...	...	...	...
121	...	11	...	29	...	...	...
122	...	15	..	...	...	...	..
123	...	14	...	...	...	...	...
124	...	12	3	...	...	..	...
125	...	12	9	..	...	..	...
126	...	13	...	...	...	...	..
127	...	13	...	..	...	...	...
128	...	16	7	16	...	...	...
129	...	12	5	29	14	1	1
130	Simpee,	14	2	15	15	7	...
131	...	16	...	...	17	...	1
132	...	14	...	...	15	6	...
133	...	12	. .	...	13	3	...
134	...	12	7	...	..	...	...

No.	Caste.	Age when catamenia commenced.			Age when first child was born.		
		Y	M	D.	Y	M	D.
135	Simpee,	13	...	...	...	...	...
136	...	13	6	...	...	...	...
137	...	14	...	...	...	...	...
138	...	12	...	...	..	...	...
139	...	15	...	...	19	4	...
140	..	13	...	...	22	...	...
141	. .	14	...	...	...	...	...
142	...	14	7	...	17	4	...
143	Khutree,	12	3	...	13	6	...
144	...	13	...	17	...	...	...
145	...	17	2	...	..	...	...
146	...	13	7	...	...	...	...
147	...	14	2	25	16	3	...
148	...	19	...	...	24	6	...
149	...	18	...	...	21	..	...
150	...	21	...	...	20	4	.
151	...	17	...	3	...	...	...
152	...	23	...	...	...	..	...
153	...	12	3	...	15	6	...
154	...	14	6	...	16	2	...
155	...	13	.	...	...	...	...
156	...	12	6	...	14	6	...
157	...	14	..	...	. .	...	...
158	...	13	4	...	15	...	...
159	...	15	...	...	...	...	...
160	...	11	9	...	14	...	...
161	Sootar,	12	2	11	..	...	...
162	...	12	...	27	...	...	...
163	...	13	3	...	...	...	...
164	...	11	7	...	...	...	...
165	...	11	11	19	...	...	...
166	...	15	...	..	19	...	...
167	...	14	..	...	16	2	...
168	...	17	...	...	19	2	...
169	...	16	2	...	18	1	20
170	...	13	3	21	14	1	...
171	Bhunsalee	14	...	13	16	...	...
172	...	17	...	19	...	...	...
173	...	13	3	...	15	2	..
174	.	11	9	23	...	...	...
175	...	13	...	...	15	3	...
176	...	14	...	...	19	..	...
177	...	15	...	...	19	...	...
178	..	14	...	...		...	..
179	...	15	...	...	17	8	...
180	...	13	2	9	...	...	...
181	...	14	..	...	..	...	...
182	...	16	...	...	..	...	...
183	...	11	3	13	15	...	...
184	..	15	2	21	17	...	...

No.	Caste.	Age when catamenia commenced.			Age when first child was born.		
		Yrs.	M	D.	Yrs.	M	D.
185	Bhundsalee	12	3	...	...	...	...
186	..	17	...	21	19	...	...
187	...	13	3	22	...	...	...
188	...	19	3	17	25	...	...
189	...	21	...	.	...	...	...
190	...	15	...	...	...	...	...
191	Muratha,	14	...	...	...	...	...
192	...	12	...	..	...	...	...
193	...	15	...	...	...	...	..
194	...	11	4	...	...	...	...
195	...	12	...	...	...	..	...
196	...	13	...	...	...	...	...
197	...	12	...	...	15	...	..
198	...	15	...	...	20	...	...
199	...	14	...	...	...	...	...
200	...	12	...	...	17	...	...
201	...	17	...	...	22	...	...
202	...	19	..	...	25	...	...
203	...	13	3	...	...	...	...
204	...	12	10	...	16	9	...
205	Bunduree,	14	7	...	21	8	...
206	...	13	9	...	16	...	
207	...	16	5	29	19	10	..
208	Mungalee,	12	6	17	14	3	13
209	...	15	...	8	18	11	21
210	Parsee,	12	...	...	14	6	21
211	...	12	2	...	16	9	...
212	...	13	...	...	...	...	...
213	...	11	8	...	17	. .	...
214	...	11	5	...	...	...	...
215	...	13	...	...	18	7	11
216	...	14	2	...	21	...	28
217	...	12	3	...	25	3	28
218	..	14	6	15	23	6	...
219	...	17	. .	22	...	...	..
220	...	12	6	11	15	3	9
221	...	12	2	...	...	...	...
222	...	11	1	3	...	...	...
223	...	13	3	7	15	...	...
224	...	11	...	9	...	...	...
225	...	11	9	22	13	11	...
226	...	12	6	21	...	...	...
227	...	11	2	...	...	...	...
228	...	14	...	...	...	...	...
229	...	14	...	...	18	4	...
230	...	13	8	...	15	4	...
	Total,	3101	3	10	1723	3	10
	Mean,	13	5	24	16	10	22

From the foregoing table, and from tables published in several former papers, I have drawn up the following summary of the results.

Earliest age in the table at which catamenia commenced, 10 years.
Latest . . . . 23 ...
Earliest . . a child was born, 12 ... 6 months.
Deduct for uterogestation, 9 months, gives mean age at impregnation, 16 years 1 month 22 days.

Puberty in Hindustan and in England.					Ages of 265 Hindu mothers at the birth of a first child.	
	Bengal.	Deckan.				
Ages.	Calcutta.	Bangalore and Bombay.	Both.	England.	Ages.	Calcutta, Bangalore, and Bombay.
8	3	...	3	...	10	1
9	7	1	8	3	11	7
10	14	4	18	14	12	15
11	37	41	78	19	13	30
12	66	71	137	35	14	54
13	49	74	123	66	15	50
14	41	46	87	99	16	31
15	11	29	40	104	17	25
16	6	16	22	85	18	13
17	3	11	14	54	19	13
18	1	2	3	34	20	6
19	...	3	3	16	21	4
20	1	...	1	8	22	4
21	...	2	2	2	23	2
22	...	...	...	1	24	1
23	...	1	1	...	25	3
					26	1
	239	301	540	540*	30	1
					31	1
	12 years, 6 months.	13 years, 4 months, 26 days.	13 years.	14 years, 10 month	32	1
					39	1
					50	1
						265

The distinguishing peculiarity with respect to the period of puberty in Hindustan as compared with the same in England consists, not in the greater frequency of precocious *puberty*, (that is, puberty under the age of eleven,) in the former, but as the table shows, in the large proportion of women who reach this epoch of life at the ages of twelve and thirteen years. Very different is the case in England; where the graceful variety of nature, if one may say so, is properly maintained. A single glance at the tabular results suffices to show how disproportionate a number of instances occurs in the two years above mentioned among the Hindus, and again how differently the figures in the English column are distributed. Instead of being largely accumulated at particular ages, they are much more evenly spread out in certain proportions through a considerable number of years—from nine to twenty-one.

In regard to the high proportion of cases of menstruation *under eleven* in Bengal, it is to be borne in mind that we have been warned, as was mentioned in a former paper concerning these, both by M. Gupta and Professor Webb, that owing to the consummation of marriage taking place in Bengal, *as a rule*, before the change which denotes puberty, they are to be received with suspicion—in other words, that probably a proportion of them are not real cases of menstruation. Taking this into account, perhaps as large a proportion of women reach puberty under eleven in England as in Hindustan. I find that the percentage under eleven is for England 3.15, and for Hindustan 5.37; but when we take the Deccan by itself the percentage of cases under eleven is only 1.66, while for Calcutta it is 10.04. Now if we reject some of the instances of puberty entered at eight, nine, ten, and eleven years in the column for the latter city, as it would seem we are warranted in doing, the result will be that the eruption of the catamenia under the age of eleven years is proportionably as common, to say the least, in England as it is in India.

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* Reduced, in order to a more accurate comparison between England and India, from the table of 2169 instances for England, given in a former paper.

May we not venture to affirm, that the occurrence of puberty, in so great a number of cases in India, at the ages of twelve and thirteen, is a deviation from the established harmony of nature? This is a question which must force itself on the attention of the most careless thinker. To me the present seems an instance analogous to what would happen were a large proportion of the youth of a particular country found to reach a certain stature about the same identical age;—when, instead of a pleasing, endless, variety in form and height, we should find a large multitude precisely alike in these respects.

It would doubtless be natural, in the case supposed, to search for the grounds of so remarkable a singularity, to enquire whether this was to be attributed to nature's laws, or to some absurd interference with those laws, which we know, by experience, man may, if he pleases, thwart, and so produce monstrous results. Just in the same manner one feels impelled to investigate in the present case. If I ascribe this Hindu peculiarity, as I confess I do, not solely to the laws of nature, but to other controlling circumstances, what, it will be asked, are those circumstances? To this my reply is, that I attribute so curious a physiological phenomenon, as stated on a former occasion, to the practice of infantile marriage that for thousands of years has existed in Hindustan, and which has gradually, in the lapse of time, produced the change. In what manner this particular social practice (great social evil I would call it) has wrought so singular a change in the Hindus, it might require a rather lengthened disquisition fairly to develope. Nor probably would the attempt be in the issue altogether satisfactory either to myself or my readers. I choose, therefore, at present to state it as an opinion which I have formed after some consideration, and leave the grounds of it to be unfolded at a future opportunity. To illustrate meantime, still further, the hitherto little understood and most preposterous condition of domestic life in Bengal in respect of marriage, I present a brief analysis of an article to be found in the third number of the Calcutta Review of October 1844, entitled "The Kulin Brahmins of Bengal." What adds so greatly to the value of this learned and ably written article, is the fact, that it is the unaided production of one who not many years ago was himself a Kulin Brahmin, but is now a minister of the Church of England.*

At a remote period a king of Bengal, to cure a feud which had sprung up among the Brahmins, selected certain who had distinguished themselves by learning and good manners, and conferred upon them the honourable appellation of *Kulins.* The Kulins thus created are like privileged families elsewhere, of divers orders, and transmit their honours in hereditary succession; nor are these honours of an empty kind, for they bring along with them important temporal advantages. Moreover, a Kulinhood is not subject to forfeiture for personal delinquencies; "the cow," they say, "does not forfeit its superiority even if it take the most filthy food, nor can the swine partake of the cow's sanctity even if it feed on grass, herbs, and water." The only sin which soils Kulinism, and this not much neither we shall find, is an *unequal marriage.*

Now, Kulins, as has just been said, are of different ranks, and the law determines that a Kulin cannot marry women from any other family, not even from the subordinate Kulins themselves, without degrading his offspring. There is, however, one exception—a Kulin, without prejudice to his rank, may marry the daughters of Shotreyas, who are a particular kind of Brahmins. When Kulins marry otherwise than as prescribed their Kul is dissolved or broken, which event, nevertheless, in these degenerate times is extremely common, and brings with it little or no disgrace. In point of fact, the male Kulins for a good bribe will, and constantly do, marry the daughters of inferior Brahmins. The less dignified Brahminical orders are ever in search of Kulin husbands for their daughters, and they well know that money can prevail even over the pride of hereditary rank; and thus it happens that a Kulin will sometimes make a handsome fortune merely by selling the honour of his hand to a number of wives of an inferior rank. In this manner Kulinism becomes the hotbed of polygamy.

The case of Kulin females is altogether different; the laws respecting them are stringently maintained. On no account must they be given in marriage but to a husband of equal or superior rank. This is a law the operation of which, as may well be conceived, is extremely hard and cruel; for since every Kulin female must be married, the rule restricting her to her own or a higher rank for a match, throws her, as a thing of course, into the arms of a polygamist, who may perchance possess (for this has been known) one hundred and eighty wives.

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* The Editor of the Review writes: "Viewed as the unaided work of a native of Bengal, the article, apart from its intrinsic merits as the best and most elaborate essay yet written on a subject of deep interest to the friends of humanity, will be considered not only a literary curiosity, (better composition have we rarely seen out of a writer's own vernacular,) but a cheering evidence of good work actually done, and a proof of what may be done—what we hope in good time will be done, by well directed educational efforts to change the nature of the people among whom we are permitted by Providence to dwell."

But a Kulin father has no alternative. The institutions of Hinduism denounce the fiercest anathemas against him should he neglect to get his daughter married before she completes her *tenth* year. The most meritorious way of disposing of her is to present her to the hymeneal altar when she is *eight* years old. The second best way is before her *ninth* year is terminated. At all events, her wedding should not be delayed beyond her *tenth* year. Longer procrastination entails upon the delinquent the guilt and infamy of *infanticide.** The distress of a Kulin parent, who happens not to be rich, when a daughter attains the prescribed marriageable age, is not to be expressed. He must find her a husband of her own or a superior rank. But without a large dowry none such are to be had. The case is urgent: his only resource is to entreat some aged Kulin to commiserate the misfortunes of an indigent fellow-dignitary, and by adding to his list of wives gratuitously a piteous girl, to save a titled family from impending disgrace. In this dilemma fathers have been known to present their daughters to Kulins on their deathbeds, in order, by marriage, to escape the condemnation of the Shastras for leaving them destitute of the connubial sacrament.

If the woman is worthy of pity, little less so is the male Kulin. "Before he is full fourteen, often when he is still younger, the troth is made in his name, the treaty signed, the ceremony performed, and the boy of fourteen is tied to and is made to sleep with a girl of eight"!†

The accomplished convert from Kulinism, our authority for these surprising details, assures us that infidelity among the numerous wives of a Kulin is probably unknown. The women, neglected and unhappy as they may be, consider matrimonial faithfulness as their first and paramount duty. It is, in truth, the only virtue which they care to preserve, and to its maintenance their whole hearts are devoted; a fact which redounds to the credit of human nature in such melancholy circumstances, though but little to the credit of the Hindu mind, whose immemorial customs, so monstrous and iniquitous, might seem to render virtue in a Kulin wife all but impossible.

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* See the Baboo Modusoodu Gupta's evidence as to what is said in the Shastras on the absolute necessity for the consummation of marriage taking place prior to puberty, in this Journal, No. 164.

† Calcutta Review, No. iii. page 22.

These details, it must be remembered, refer to the most dignified class of persons in Bengal; in whom, if in any portion of society, some traces of sense and wisdom might be expected to be found. But such are the institutions of Hinduism; and so vicious are they, that there can be no hope for India but in their destruction. A change to be effected not by force or by fraud, however; but in part by immediate prudent legislation, and at length completely by the introduction of that great source of beneficent innovation, unadulterated Christianity, with the arts, sciences, literature, and mental freedom that follow in its train.

ART. IV.—*On Fractures of the Cervix Femoris, with Cases.* By ALEXANDER WATSON, M. D., F. R. C. S. E., Consulting Surgeon to the Royal Infirmary, &c. &c.

FRACTURES of the *cervix femoris* were long involved in confusion and obscurity, both in regard to their diagnosis and results. But modern investigation has completely elucidated the nature, variety, and diagnosis of these injuries. We are chiefly indebted for this to the observations of Sir Astley Cooper, which have left very little to be added to this very important and interesting subject. In regard to their results, however, there is still one question which remains to be settled, whether, in regard to fractures within the capsular ligament, a complete osseous re-union ever takes place. Any new facts, therefore, bearing upon this important question cannot fail to prove interesting to the practical surgeon.

My attention having been particularly directed to this subject some years ago, I have examined carefully a very considerable number of cases and preparations, in which the *cervix femoris* had been fractured. From these cases I have selected those I am about to detail, as tending to illustrate the still undecided question above-mentioned.

It will be seen that, in the prosecution of this examination, I have been greatly indebted to several of my professional friends for the use of cases, which they have on all occasions given in the most prompt and liberal manner.

In proof of the difference of opinion which exists, and the doubts entertained, in regard to the possibility of fractures of the *cervix femoris* reuniting by bone, I may here give a short statement of the views of the chief authorities on the subject.

The late Mr John Bell and his brother Sir Charles were of opinion that fractures of the *cervix femoris within* the capsular ligament did not reunite by bone; if *exterior* to it they did.*

Mr Earle considers reunion by bone possible under favourable circumstances.†

Mr Amesbury is of opinion "that fractures of the *cervix femoris* within the capsular ligament, unattended with laceration of the close coverings, will unite by the intervention of bone, if apposition, pressure, and rest be maintained;" but not so rapidly as if in the middle of the bone.‡

Dr Colles thinks that ligamentous union only takes place.§

Mr Abernethy stated in his lectures, regarding reunion by bone, of fractures of the *cervix femoris* within the articular ligament, that this "certainly is a case of rare occurrence, and no positive proof has as yet been offered to us of any such union taking place. I hope, however," he added, "such proofs will arise in the after part of surgery, for the honour of the profession."‖

Mr Langstaff says, "that the arteries belonging to the hip-joint are capable of producing bony union within the capsule, there can be no doubt; nor can there be any doubt that this would take place, as in other fractures, if the parts could be kept steadily in juxtaposition, and a sufficient length of time were allowed to make the union complete."¶

Baron Boyer gives a very guarded opinion on this subject, and although he states that fractures of the *cervix femoris* may reunite under favourable circumstances, he seems to do so only in regard to those at the base of the cervix, (exterior, therefore, to the capsular ligament,) and in young subjects.**

B. Larrey entertained no doubt as to the possibility of reunion of fractures within the capsular ligament by bone.

B. Dupuytren says, "Pour nous, nous regardons comme démontrée cette consolidation, quoi qu'en disent les chirurgiens Anglais, et quelque grande qu'ait put être jusqu'à present, notre propre incrédulité à cet égard."††

Mr Liston and Mr Stanley have shown their belief in the possibility of the bony reunion of these fractures, by the cases which they have published in order to prove this, and which are afterwards to be noticed.

Sir Astley Cooper, who was long justly considered the highest authority upon this subject, continued to entertain to the last the opinion which he had expressed at an early period against the reunion of fractures of the *cervix femoris*, when situated within the capsular ligament. He did not venture to deny the possibility of this; but he had never been convinced of its occurrence by any case he had ever seen.*

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* See Principles of Surgery; Observations on fractures of the spine and neck of the thigh bone; and Institutes of Surgery.

† Practical Observations in Surgery. Lond. 1823.

‡ On Fractures. Lond. 1831.

§ Dublin Hospital Reports, vol. ii. 1818.

‖ Lectures on Anatomy and Surgery. Lond. 1828.

¶ Lond. Med. Chir. Trans. v. 13.

** Traité des Mal. Chirurg. T. 3.

†† Lec. Orales de Clin. Chirurg. T. 2. Paris, 1832.

Professor Chelius of Heidelberg says, "The opinions of surgeons on the prognosis in fractures of the neck of the thigh-bone are very different." After stating that of Sir A. Cooper, he continues,—"These reasons, which had even earlier been advanced, were contradicted by Boyer, and more recently by Earle. Experience also has proved that bony union may take place in these cases. That this has hitherto in England been little observed may depend, on the very careless treatment of fractures known to be within the capsular ligament. The condition of the head of the thigh bone has nothing to do with it, but rather the difficulty of keeping the fractured ends for a proper time in sufficient contact; frequently in complete tearing of the fibrous covering of the neck of the thigh bone, or in bad constitution or old age of the patient, is the union of the broken ends often protracted or entirely prevented."†

The difference of opinion here stated has arisen in some measure from a want of precision as to the exact nature of the cases referred to, and the want of unquestionable proof by dissection. Some of the inferences have evidently been drawn from insufficient grounds, such as cases of apparent fracture of the *cervix femoris*, which had recovered so far as to admit of the patient's walking about; but the exact situation of the fracture, and whether united by bone or ligament, were not ascertained. The only cases, however, from which light can be thrown upon this subject are those, in which the history and phenomena attending them were observed from their commencement, the progress of the cases known, and the parts carefully examined after death. Hence, it is only to such cases we can refer in proof of the point now under consideration.

From what has been stated it may be inferred that the most common opinion, in regard to the result of fractures of the *cervix femoris*, is that, when the fracture is situated without or *exterior* to the capsular ligament of the hip-joint, reunion by bone may be expected; but when the fracture is *within* the capsular ligament, reunion by bone is scarcely to be looked for, because sufficient proof of the possibility of this result has not yet been established.

It is to be regretted that many of the most interesting cases of this injury, in which the patients had recovered and survived many years, had not been examined after death. In such cases, it has been most common to infer that, as the patients recovered with the use of their limbs, the fracture must, therefore, have been *exterior* to the capsular ligament; and, in some cases, even the existence of a fracture has been doubted. This has evidently arisen from the great unwillingness of surgeons to adopt an inference at variance with preconceived opinions;—a case precisely parallel to that entertained of syphilitic sores a few years ago, when of all those which got well without the use of mercury, it was said, that "they could not be syphilitic."

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* Treatise on Fractures and Dislocations, 8th edition, p. 122.

† Chelius' Surgery, p. 566, § 672, translated by South. London, 1845.

In treating of fractures of the *cervix femoris* Sir A. Cooper was the first to give greater precision to the different modifications of this injury than other authors had done. He has described there distinct modifications, the accurate discrimination of which is of great importance. These are, 1. When the fracture is through the *cervix femoris* and *within* the capsular ligament of the hip-joint. 2. When the fracture is at the lower extremity of the cervix and *exterior* to the capsular ligament. 3. When the fracture is through the trochanters of the *os femoris.*

But strictly speaking a fracture may take place through the trochanters, separating the head and neck of the bone (along with more or less of the trochanters) from the shaft, without the cervix being actually fractured. Such cases, therefore, do not properly come under consideration at present; and the only distinction which requires to be made is in regard to fractures situated *exterior* to, or *within*, the capsular ligament of the joint.

The following table gives a statistical detail of the cases of fracture of the *cervix femoris* which I had examined in Edinburgh a few years ago. It shows the comparative frequency of the different modifications of this injury and their results.

	Total.	Fracture situated within capsular ligament			Fracture situated exterior to capsular lig.		
	No. of cases.	No. of cases.	United by bone.	Not unit. by bone.	No. of cases.	United by bone.	Not unit. by bone.
University museum,	6	2	0	2	4	2	2
Col. of Surgeons' museum,	21	11	1	10	10	9	1
Dr Robertson's collection,	10	3	1	2	7	7	0
Dr Handyside's collection, including that formerly belonging to Professor Lizars,	11	5	1	4	6	5	1
Professor Fergusson's collection, now in London,	7	4	0	4	3	3	0
Dr Watson's collection,	6	3	0	3	3	1	2
Mr Bell's case,	1	1	1	0	0	0	0
	62	29	4	25	33	27	6

NOTE. This table includes several recent cases in which the patients did not survive a sufficient length of time for reparation. In some of the other cases the process of cure was only in progress.

In regard to cases of fracture of the *cervix femoris* situated exterior to the capsular ligament, there is now no doubt as to their termination by osseous reunion. But in consequence of the present unsettled state of the question of osseous reunion of fractures within the capsular ligament, an account of the following cases may prove interesting, as tending to illustrate the subject.

It is unnecessary to describe here the more common terminations of this injury which have been observed,—namely, by absorption of the neck of the bone, a false joint, and ligamentous union, which have been so often and so well described by the highest surgical authorities. I shall, therefore, proceed to describe four cases of apparent osseous reunion of fractures situated within the capsular ligament, the preparations from which I have carefully examined.

Case I.—For this case I am indebted to Dr J. A. Robertson, in whose museum the preparation of the bone is contained, and who has been kind enough to furnish me with the following history of the case.

" A. G. aged 54, a bricklayer, fell, twelve years before his death, from a scaffold about 18 feet in height, landing on the heel of the left limb. Fracture of the *cervix femoris* occurred, as indicated by slight crepitus, and shortening of the limb to the extent of about one inch. On causing rotation of the limb and grasping the *trochanter major* the femur appeared to revolve on its own axis.

" He was so much injured otherwise by the concussion of the fall that he had palsy of the limbs and bladder for a length of time, but from which he gradually got well.

" He was kept still in bed for a considerable time, without bandaging, and recovered the use of his limb."

On examining the *cervix femoris*, it is much shortened, has new depositions of bone projecting from its anterior surface, and an apparent deficiency from absorption at the posterior part. The internal perpendicular section through the head and neck of the bone exhibits a distinct line of solid osseous matter, uniting the head to the remaining part of the cervix in connection with the shaft of the bone.

Case II.—This case occurred in the practice of Mr Lizars, to whom I am indebted for the history of the case.* A cast was made from the case soon after the accident, and the head of the bone was obtained after the patient's death, which took place seven years from the receipt of the injury. These preparations are now in my possession.

The patient was a shoemaker, 63 years of age when he received the injury. He had all the symptoms of fracture of the *cervix femoris*, and was treated accordingly. The flattening of his hip and elevation of the trochanter were so well marked, that Mr Lizars used the cast made from the patient to illustrate the subject at his lectures. The patient afterwards recovered so well that he was able to walk about.'

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* This case is alluded to and figured by Mr Lizars in his Surgery, vol. i. p. 145.

On dissection, the bone exhibits appearances externally of fracture and deposit of new bone at the upper part of the *cervix femoris*, upon its anterior surface, which was included within the capsular ligament. The posterior part of the cervix is much shortened by absorption and the altered position of the head of the bone, but the whole is completely consolidated.

The horizontal section of the bone exhibits the altered position of the head, arising obviously from the curved form of the cervix, by which it has become convex anteriorly, and concave posteriorly. The new and old parts of the bone having assumed an uniform cancellated structure, no distinct line of osseous reunion is here observable.

Case III.—The following case was published by Dr Begbie in the Edinburgh Journal of Medical Science; but it bears so strongly on the point at issue that I insert a short account of it here.*

"A feeble lady, aged 77, (November 1821,) in walking from her bed-room to an adjoining apartment, slipt her right foot suddenly and unexpectedly on the edge of the landing place of a flight of steps, and immediately fell or sunk down on the right hip. She attempted to rise, but found she had completely lost all command over the limb of that side." She was conveyed to bed and seen by Dr Begbie next morning, who found her lying upon her back, the right limb shortened about an inch and a half, the knee and toes considerably turned outwards, the heel resting in the hollow between the *malleolus internus* and *tendo Achillis* of the left leg. She felt little pain, unless when the limb was extended or rotated, so as to place the limb in its proper position. This could easily be effected, but the limb again returned to its former situation. "No crepitus could be distinguished in the recumbent posture," and no other trial was made to ascertain its presence.

The limb was kept as much in its proper position as possible, by a simple bandage placed around it and the body. The patient lay for five months in bed, and occasionally removed to a sofa. She so far recovered as to walk at first with the assistance of crutches, then with a staff, and latterly, before the end of a year, with a high-heeled shoe. "The knee and foot continued very much everted, and the limb considerably shortened." In April 1824 she died from an affection of the brain, being about two years and half after the receipt of the injury.

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* The preparation from this case is preserved in the museum of the Royal College of Surgeons.

On examination after death, the eversion and shortening of the limb were still conspicuous. The head of the *os femoris* moved freely in the acetabulum, which did not appear to be diseased. The capsular ligament was considerably thickened; the *ligamentum teres* was entire and healthy.

The posterior part of the *cervix femoris* was very much absorbed, so that the head of the bone approached near to the *trochanter major.* The anterior part of the cervix is bulging and prominent, and presents the appearance of new osseous depositions close to the head of the bone.

The internal structure of the bone, as exhibited by a longitudinal section, presents the appearance of a transverse deposition of dense osseous matter crossing the cervix close to the head of the bone.

This case presents all the characters of a fracture of the *cervix femoris,* which terminated in a favourable manner. But from the apparent complete reunion of the broken part, the previous existence of a fracture was doubted by Sir A. Cooper, to whom the preparation had been shown. The only reasons he could give for his doubts were, that, in his opinion, the internal appearance did not present the characters of fracture; and that the *ligamentum teres* and cancellated texture exhibited appearances of disease.* Certainly no disease existed in this case, or disorder of the natural structures, more than might have been expected to follow a fracture of the bone. So that the opinion given by Sir A. Cooper, I cannot help thinking, arose from his extreme unwillingness to alter the opinions he had long entertained on this subject. The age of the patient,—the suddenness of the disabled state of the limb,—the manner of the injury,—the symptoms, progress, and history of the case, all tend completely to corroborate the nature of the injury, as indicated, if not proved, by dissection.

If disease did previously exist in this case, there can be little doubt that a fracture followed, which appeared to terminate in a cure with a united and useful limb. The surprise would thus be greater, than if fracture of a sound bone had united.

From the manner in which the injury was received (by a fall on the hip), and no crepitus being felt, was the neck not driven into the cancellous structure of the head of the bone?

CASE IV.—This case was given to me by Mr B. Bell, which I here insert in his own words.

"Mrs W—— was 67 years of age when she met with the accident, in summer 1835. It happened, according to her daughter's account, in the following way:—'She was descending from the top of a stage coach, and, being very active for her time of life, jumped down, without calculating the height. She lighted on her feet, but immediately found herself unable to stand, and fell to the ground. When raised up, her leg turned quite round, and she had no power to keep it in its proper position. She was brought home in a coach and laid upon her bed. When Mr Bell called, he said that the neck of the thigh bone was broken. She lay on her back for ten weeks or more with a pillow under the knees.'

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* See his letter published along with case.

"I regret that I kept no written notes of this case; but I have seen her on many occasions during the last seven years, and always regarded her as the subject of fracture within the capsular ligament. Latterly she moved about with great alacrity, unassisted by either crutch or staff, so that, upon coming to examine the joint after death, I was prepared to find a more than usually complete ligamentous union, with thickening of the capsule; but an entire bone was more than I expected. It is possible that fracture may never have existed in this case at all; but the mode in which the injury occurred, the testimony of her daughter as to the symptoms at the time, the subsequent history, and my own impressions, all lead me to believe that the *cervix femoris* had been broken."

The bone presents the appearance of a line of fracture at the upper extremity of the cervix; the same distortion of the cervix and head of the bone as in those already described above; and the same shortened and concave form at its posterior part. Although the head of the bone seems to have been detached from the cervix, it is now firmly re-united by bone.

These cases appear to me completely to prove the problem of the possibility of bony re-union of fracture of the *cervix femoris* situated within the capsular ligament of the hip-joint.

If any reliance is to be put on the diagnostic marks of this injury which have been pointed to by all the most experienced surgeons, and which are said by them to render the nature of the case indubitable, then, certainly, these cases, previous to anatomical examination, must have been considered fractures of the *cervix femoris.*

The question then comes to be, Does the anatomical examination of these cases confirm this opinion? The appearances presented by these four specimens are in several respects very similar. 1. The head of the bone seems to have been detached from the cervix; 2. Much absorption of the cervix has taken place; 3. The distorted position of the head and cervix is the same in each of them; 4. The distorted shape of the cervix at the point of union is precisely such as might be expected to take place in consequence of the everted position of the limb during the process of reunion, while the head of the bone remained fixed in the acetabulum.

From these circumstances, taken along with the previous history of the cases, which were not only similar, but presented the usual indications of fracture, it certainly appears to me obvious that the only proper conclusion which can be drawn is, that they were cases of fracture of the *cervix femoris* within the capsular ligament, reunited by bone.

But these are not insulated cases tending to establish this circumstance. The cases so well described by Mr Langstaff,* Mr Stanley,† and Mr Liston,‡ afford strong evidence in proof of the same proposition.

It is unnecessary to cite here the case of Mr Liston, which is given in the 16th volume of this Journal, p. 212. But as a very interesting case was published by Mr Stanley in the Med.-Chir. Trans. (vol. xviii. 1833,) in which complete bony union seemed to have taken place after fracture of the neck of the femur within the capsular ligament, a short abstract of it may be proper.

The patient, a youth 18 years of age, fell from a loaded cart upon his right hip, by which he was unable to move the limb, and suffered severe pain when it was moved by another person. The thigh was bent to a right angle with the pelvis and could not be extended. Abduction of the thigh was difficult. The limb was everted, at first slightly, afterwards in a greater degree. The soft parts around the hip-joint were considerably swollen. There was no shortening of the limb, which rather appeared to be lengthened. No crepitus could be felt in any movement of the limb. Extension was applied to the limb on the supposition of there being a dislocation.

About two months after the accident the patient was admitted into St Bartholomew's Hospital. His health was much disordered. He complained of febrile symptoms and pain of his hip and thigh. After having been in the hospital about a month, he was seized with small pox and died.

S. C. "In the examination of the body no other morbid appearances were discovered besides those of the injured hip-joint. The capsule of the joint was entire but a little thickened. The *ligamentum teres* was uninjured. A line of fracture extended obliquely through the neck of the femur, and entirely within the capsule. The neck of the bone was shortened, and its head, in

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[*] Lond. Med. Chir. Trans. vol. xiii. p. 499.

[†] Med. Chir. Trans. vol. xviii. and xxiv.

[‡] Edin. Med. and Surg. Journal, April 1820.

consequence, approximated to the *trochanter major*. The fractured surfaces were in the closest apposition, and finally united nearly in their whole extent by bone. There was an irregular deposition of bone upon the neck of the femur, beneath its synovial and periosteal covering, along the line of the fracture."

This accident is remarkable in so young a subject, but is easily explained by the manner of its infliction. The union is to be accounted for by the youth of the patient, and the periosteal covering of the bone having remained entire.*

In the twenty-fourth volume of the London Medico-Chirurgical Transactions a case very similar has been published by Mr Jones of Worcester, with remarks upon it by Mr Stanley. The patient had "slipped down" in October 1838. A fracture at the neck of the thigh bone was detected next day; a splint was applied. This was continued for eight weeks. In the following spring he was able to move about with a stick, but the limb was shortened one inch and a half and considerably everted. In April 1840 he died of chronic disease of lungs, aged 82, having lived a year and a half after the injury.

On dissection the neck of the bone was found to have almost entirely disappeared, and a firm bony union to have taken place between the head and the remaining part of the neck and shaft of the thigh bone.

Mr Stanley remarks that this was clearly a case of fracture of the neck of the bone, and without doubt situated within the capsular ligament, as also of osseous union.

If these were not cases of fracture of the *cervix femoris*, in what way, it may be asked, could the phenomena described have been produced? I have known cases of chronic disease of the hip-joint produce absorption and distortion of the *cervix femoris*, accompanied, in some of the cases, with irregular osseous depositions; but certainly no such cases are ever attended with similar symptoms; such as immediate loss of power, shortening of the limb, crepitus, &c. Besides, cases of this disease of the joint gradually get worse, while those of fracture get gradually better. Neither could a contusion of the joint cause this combination of symptoms without the presence of a fracture.

But although these cases appear completely to prove the *possibility* of osseous reunion in the cases referred to, this is to be considered a rare occurrence, rather than the common result of the injury; and it is one which seems to require the concurrence of several favourable circumstances for its accomplishment.

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* This case is quoted in South's translation of Chelius. The preparations from this and the following case are preserved in the museum of the St Bartholomew's Hospital.

51, *Queen Street, Edin. May* 1846.

Art. V.—*On Malarial Fevers as observed in Canada from 1838 to 1846.*[*] II. *Analysis of repeated attacks in the same individual.* By Thomas Stratton, M. D. Edinburgh; Honorary Member of the Highland Society of Canada; Corresponding Member of the Literary and Historical Society of Quebec; Navy Medical Department.

While the paroxysms of intermittents show generally some regularity in the periods of their accession in the course of the same illness, it may be of some practical importance to inquire, in cases where a person has had repeated seizures, whether the attacks evinced any regularity in the periods of their approach. The results of such an inquiry might determine whether there was here any law of habit against which it was proper for the person to take prophylactic measures, or whether the second and following attacks accidentally arose only from re-exposure to the same causes which produced the first one, (with the predisposition added.)

Case 1.—At Grand River, Lake Erie, in 1842, Mr G. S., an intelligent proprietor and farmer, informed me that he settled there in 1823, and for nine years, till 1832, had marsh fever in some one of its forms every autumn for two or three months, being often confined to bed and sometimes dangerously ill. In 1832, before August, the time when he was generally taken ill, he went to a healthy part of the country and did not return till the fever season was over. In this year he had no fever, and has not had any for these eleven years since, though living at the same place, where there is still about as much malaria as before. Did no annual attack appear in 1834 on account of the one for 1833 having been prevented? Is there sometimes in disease a habit as regards seasons, which in this case was broken?

Case 2.—I shall now give a brief account of all the attacks of malarial fever in the same individual that have occurred up to the present time. Twenty of them took place in Canada and one in Scotland, and of them I may add that I myself was the subject.

Attack 1. At Port Colborne, Lake Erie, 1840, Dr Stratton, aged 23, two and a half years in various malarial parts of Canada; never had malarial fever before. July 25 to August 27, ill of malarial continued fever.

July 25 and 26. Without previous rigours; there was languor, lassitude, morbid heat of skin, tongue and mouth dry, thirst, urine scanty, pulse feeble and quick, bowels confined.

July 27. Vertigo, extreme weakness, unable to sit up in bed.

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[*] Concluded from Vol. lxiv. p. 115. See also the Edin. Med. and Surg. Journ. for April 1841, October 1843, April 1844, and April 1845.

During the whole day the mind was full of a rapid succession of strange, unconnected, and incomplete ideas on all kinds of subjects; then came a feeling of falling off, as if he were about to cease to exist; then, by a mental effort, he was astonished at these extraordinary ideas, and for a minute or two was free from them, when the same process went on again. If he had expressed these thoughts in words, it would have been delirium; as he was silent, it was a kind of "*delirium mutum.*" Once or twice when spoken to, he was roused from the wandering state of thought; but when the exertion of answering was over, he fell back again into the same condition. The 27th was the only day he was in this state. There was generally headach at night; was much increased by any noise. He observed the greatest difference between the effect of the noise of persons speaking or reading, and any other, and even much louder noise, such as that made by workmen or machinery. The former, addressed to the mental faculties, increased the headach much more than the latter. There never was any delirium. On several days, wandering pains were felt in the left side of the chest, thighs, and abdominal muscles. On August 23, he went to another part of the country 240 miles distant, and after this change of air he got well about 27th August.

There were no fits of ague towards the end of the attack, and no lip-eruption. Chronic rheumatic pains in the shoulders, elbows, knees, and ankles were troublesome for some months after.

The next three attacks were at Kingston, Lake Ontario.

Attack 2. Five months and a-half after there appeared quotidian of two fits, the second wanting the sweating stage.

February 20, 1841. At 7 A. M. pain in the head and eyeballs for two hours. At 9 A. M. the first stage began and lasted eight and a-half hours, there being coldness in the bowels; skin cold; shivering; dyspnœa; cough; pains in the head, elbows and loins, and vomiting of greenish fluid. In the hot stage, which lasted five hours and a half, there was less languor, weakness and headach, and he felt less uncomfortable than in the first stage. In the sweating stage, which lasted an hour, the headach disappeared. He had not energy enough to observe the pulse during the paroxysm. Purgative medicine.

February 21. One stool. Ten grains of quinine in the afternoon; felt uncomfortable and aguish; and at 5 P. M. the cold stage without shivering lasted three hours. The hot stage lasted eight hours, from 8 P. M. to 4 A. M. on the 22d.

February 22. Fell asleep at 4 A. M., and awoke at five by a constant dull pain over the whole abdomen, increased on pressure. Two hours after, it became remitting, and continued so for the next two days, and was treated with laxatives, opiates, and fomentations. It perhaps arose from the bed-clothes being mostly thrown off in the hot stage, and the room being cold. No more fits. No lip-eruption. 26th, Convalescent.

Attack 3. A fortnight after there appeared tertian, of which there were three fits, followed by two paroxysms of quotidian.

March 10. At 12 A. M. the cold stage lasted five hours, and was succeeded by the other two stages.

March 12. At 11 A. M. the first stage lasted eight hours, and was followed by the other two stages.

March 14. Had the premonitory feeling of a fit for half an hour. At 8½ A. M. the first stage continued for two hours, with yawning, coughing, chills, and shaking. The hot stage, with vomiting, lasted an hour, and was followed by the sweating stage. Not much headach in any of the stages.

March 15. At 3½ P. M. chills and shaking for two hours; then hot for an hour; no sweating stage.

March 16. Had the premonitory feeling of a fit for an hour, and then at 8½ A. M. cold and shaking for two hours, hot for two hours, and then a partial sweating stage. In the first stage took half a drachm of the tincture of opium, and had no headach during the fit, but some after it. The cold and hot stages felt rather pleasant than otherwise. Took twelve grains of quinine during the day. On the 15th a lip-eruption appeared, and lasted eighteen days, till the 2d April.

Attack 4. A fortnight after, there appeared tertian of two fits.

April 3. Got his feet wet the day before, and at 12 A. M. the first stage lasted two hours with coldness; no *cutis anserina*; yawning; occasional cough; deep breathing; the urine copious and pale. At the beginning of the stage unusual brightness of ideas; towards the close, headach and redness of the conjunctivæ. The hot stage lasted an hour and a half and the sweating stage an hour.

April 5. At 9 A. M. the first stage lasted two hours, with chills, shaking, chattering of the teeth, yawning, oppression of the chest, and quick breathing. The hot stage lasted an hour and a half, and the sweating stage three hours. Extreme weakness and violent headach throughout the fit, which was much more severe than the last. Four grains of quinine.

April 7. Great weakness; no appetite. Nine grains of quinine. Escaped the fever.

April 8. Eight grains of quinine. A lip eruption.

April 9. No fit since the 5th. Six grains of quinine.

Attack 5. Seven weeks after in Edinburgh there appeared tertian of two fits.

May 27, 1841. Bathed in the Firth in the evening, and then chilly and (aguish) yawning for some time after.

May 28. After half an hour of premonitory symptoms, the first stage at 12½ A. M. lasted an hour and a half, with coldness outwardly and inwardly, shaking, stretching of the arms, and a desire for many various kinds of drink disliked as soon as tasted. The second stage lasted an hour, with great heat, headach, dyspnœa, and thirst; drank eight imperial pints of water. The third stage lasted half an hour.

May 29. Ten grains of quinine.

May 30. At 11½ A. M. the first stage lasted an hour and a half; the second two hours and a half; and the third half an hour.

May 31. Twelve grains of quinine. June 1. Escaped the fit. No lip eruption.

The next three attacks were at Kingston.

Attack 6. Four months and a half after there appeared monoparoxysmal (incomplete) ague, the hot and sweating stages being deficient.

October 18. At 7½ A. M. felt for three and a half hours languor, weakness, anorexia, and other premonitory signs of a fit. At 11 A. M. the cold stage lasted ten hours, till a warm bath was taken, after remaining in which for about ten minutes, there was no more of the cold stage and no hot or sweating stage. No other fit.

Remarks.—If there had been any regular hot stage here, it would have shown itself after leaving the bath. As no quinine or other medicine was taken, it was not checked by quotidian or tertian, but a natural instance of ague of one fit; its recovery without any treatment illustrates the natural tendency (at times) of some diseases to spontaneous recovery, which is to be borne in mind when considering the efficacy of remedies.*

Attack 7. A month after. There was incomplete and colic ague for a day.

November 21. After wet feet in the morning, there were in the afternoon slight tormina and tenesmus. At 6½ P. M. a cold stage, with chills, shaking, dyspnœa, and sighing, lasted two hours and a half, till a warm bath was taken. No hot or sweating stage. In the night abdominal pain, increased on pressure; the pulse quick, not hard or full; some vomiting; one stool. Hot fomentations; several doses of laxative medicine were rejected, and a draught of *Tinct. Opii*, m. xxxv. and *Spirit. Æther. Nit.* ʒss. was retained.

November 22. Vomiting spontaneously, and after various medicines taken; occasional abdominal pain and griping. Hot fomentations.

November 23. Pain as before; urine scanty and high-coloured, and pain in the bladder on micturition; pain in the region of the spleen, particularly when lying on the right side; five stools. Hot fomentations and the turpentine epithem.*

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* See Dr Alison's Pathology and Practice of Medicine, 1843, page 74.

November 24. Convalescent.

Attack 8. Six weeks after there was latent or incomplete ague for a week, with a fit on one day.

January 7, 1842. Got wet feet, and at 4 P. M. great weakness in the back and legs; anorexia, languor, and an aguish feeling till bed-time.

January 8. From 6 A. M. the above symptoms and headach, chilliness, no shivering, and pains in the shoulders and elbows, continued for nine hours till a warm bath was taken. No hot or sweating stage.

From January 9 to 13. Languor and anorexia; great weakness in the loins and legs, and wandering pains in the limbs.

Attack 9. Seven months after, at Grand River, there appeared quotidian of four fits, the first two being incomplete.

August 10. Has been two months at Grand River; got wet feet the day before. After having the premonitory signs a fit for an hour at 1 P. M.; chilly and shaking for three hours; no hot or sweating stage.

August 11. At 2 P. M. languor and lassitude, and rather chilly for two hours; no hot or sweating stage.

August 12. Premonitory symptoms for half an hour, and then, at 11¼ A. M., the first stage lasted an hour, with coldness at first inwardly, then outwardly; shaking and vomiting; the second stage lasted 2½ hours, and the third two hours. Headach throughout the fit, and for some time after it.

August 13. At 7 A. M. eight grains of quinine in water and sulphuric acid; at 2 P. M. the fit commenced, each stage lasting an hour.

August 14. Eight grains of quinine; escaped the fit.

August 15. A lip-eruption which lasted 14 days; four grains of quinine.

Attack 10. Five weeks after at Grand River there again appeared quotidian, its first and second fits wanting the third stage.

September 20. Had bathed the day before, and at 11 A. M. the cold stage had lasted three hours; then a hot stage of two hours, and no sweating stage. Four grains of quinine.

September 21. Four grains of quinine. At 12 A. M. the cold stage lasted three hours and a half, and the hot stage an hour: no sweating stage.

Took 13 grains of quinine in the next two days; no other fit; no lip-eruption.

The next eight attacks were at Kingston.

Attack 11. Four months after there was latent or aparoxysmal ague for six days, there being no distinct fit.

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* See Dr Copland's Dict. of Medicine, ii. 410.

January 22, 1843. At 6 P. M. weakness, languor, and anorexia all the evening.

January 23. Last night no sleep; restless; skin hot; weakness in all the large joints; a peculiar feeling of weakness and coldness in the left ankle, and as if a part of the joint were wanting; purgative medicine; three stools.

January 24. The above symptoms. Purgative medicine. He improved gradually up to the 28th, when he was convalescent. Last four days took daily five grains of quinine. No lip-eruption.

Remarks.—If the above symptoms had appeared in a person who never had intermittent and had never been in a malarious district, there would be no occasion to look upon them as signs of ague; but taking into consideration the circumstantial evidence, they seem in this instance to be such. While in incomplete ague there sometimes are paroxysms wanting two of their three stages, there are also cases of aparoxysmal ague where not one of the three characteristic parts of a fit is present.

Attack 12. Three weeks after, there appeared a single fit of ague.

February 12. Got wet feet, and in the evening felt as if he were about to have a fit.

February 13. At 3 A. M., the cold stage lasted an hour, the hot stage two hours, and the sweating stage an hour. *Calom.* gr. v. At 6 P. M., six grains of quinine. On the next three days, there were taken six grains of quinine and three of capsicum. No second fit; no lip-eruption. This was either monoparoxysmal ague, or checked quotidian or tertian.

Attack 13. Three weeks after, there was a single fit of ague.

March 5. In the forenoon had been sitting in a cold place and also got wet feet. At 2 P. M. premonitory signs of a fit for two hours; then the first stage lasted four hours, there being chills, shaking, unusual brightness of ideas, headach, pains and weakness in the back. The hot stage lasted half an hour and the sweating stage an hour. Took six grains of quinine on this and each of the next two days. No second fit; no lip-eruption. Rheumatic pains troublesome for some days after.

Attack 14. Ten days after, there was tertian of two fits, the first having only the cold stage, the second having no sweating stage but two hot stages.

March 16. At 6 P. M. the cold stage lasted two hours; no hot or sweating stage.

March 17. Felt quite well.

March 18. At 11 A. M. the cold stage lasted four hours and a half, beginning with great weakness in the back, then great coldness inwardly and outwardly, shaking, laborious respiration, stretching of the arms and legs; the urine copious and pale. At the beginning of the fit unusual sprightliness of ideas, and then headach and nausea. The hot stage lasted two hours with headach; the urine scanty and light coloured; it ended and there was no headach; and the skin was quite cool for three quarters of an hour, and no sweating appeared, when a second hot stage set in and lasted two hours, with the same symptoms as the former, and also dyspnœa, cough, and slight pain in the left side of the chest. After this there was no sweating. The second hot stage may be regarded as a misplaced hot or a rudimental sweating stage.

March 19. Face and conjunctivæ slightly yellow; objects not seen of a yellow tinge. A calomel purgative and nine grains of quinine.

March 20. Escaped the fit. In the region of the spleen some pain, hardness, and pulsation on slight pressure, and on lying on the right side and walking, a sense of weight and dragging.

March 21. Several dreams in the night; the spleen feels less heavy and distended; no beating in it as yesterday. Skin cool, appetite moderate, pulse 84. A lip-eruption, which lasted ten days. Calomel gr. v. and nine grains of quinine.

March 22. No uneasiness in the splenic region. No more fits. Took quinine for several days.

Attack 15. A fortnight after, there appeared quotidian of three fits, the two last each wanting the third stage.

April 5. At 9½ A. M. the cold stage lasted an hour. Among other symptoms there was stretching of the arms, and on stretching the left one, there was felt a dragging pain in the left mammary region, as if some inter-pleural bands were being stretched. The hot stage lasted seven hours and a half, and the sweating half an hour.

April 6. At 8 A. M. purgative medicine and five grains of quinine. At 11½ A. M. cold and shivering for an hour; hot for two hours and a-half; no sweating. Twelve grains of quinine and four of capsicum in three doses.

April 7. Twelve grains of quinine. At 9½ A. M. cold for two hours and then hot for three hours; no sweating. Two stools. Six grains of quinine.

April 8. Six grains of quinine. A lip-eruption, which lasted six days. Escaped the fit. Took nine grains of quinine in the next two days.

Attack 16. Seventeen days after there was tertian of two fits, both wanting the third stage; then three nights of remittent fever.

April 25. Slept two nights ago in a room with a window partly open; at 11½ A. M. the cold stage, without shivering or shaking, lasted two hours and a-half, and the hot stage an hour and a-half; no sweating stage.

April 27. At 10 A. M. the cold stage lasted 2½ hours; the hot stage 3½ hours; no sweating stage.

April 28. Six grains of quinine. About 8 P. M., without previous rigors, there came on flushes of heat, then the skin was hot generally and headach all night, and about 7 A. M. profuse perspiration.

April 29. During the day cool and weak. Fifteen grains of quinine in two doses. In the night a paroxysm like that of last night.

April 30. Nine grains of quinine. In the night some heat of skin followed by sweating. In the next four days took fifteen grains of quinine. No lip-eruption.

Attack 17. A month after, there was a single fit of ague.

June 3. Had been lying on damp grass the previous afternoon, and at 10 A. M. cold and shaking for two hours, hot for three hours, and sweating for one hour. After the fit took six grains of quinine; took eighteen grains in the next three days. No other fit. No lip-eruption. As quinine was taken before there was time for a second fit, this may be called checked quotidian or tertian.

Attack 18. A month after there was tertian of two fits, each having only the cold stage.

July 2. In the evening fell asleep near an open window.

July 3. At 2 P. M. the cold stage lasted three hours; no hot or sweating stage; headach and weakness all the evening.

July 5. At 4 P. M. cold and shivering for an hour. No other stages.

July 6. Well all day. 7th. At 4½ P. M. weakness in the legs, no chills, (the shadow of a fit.) No lip-eruption. No medicine taken during this attack.

Attack 19. Three weeks after, there was tertian of three fits, occurring while going in a steamer from Port Stanley to Detroit, Goderich, &c.

August 19. Got wet in the afternoon.

1843.	Cold Stage.			Hot Stage.			Sweating stage.			Paroxysm.		
	From	To	Lasting hours.	From	To	Lasting hours.	From	To	Lasting hours.	From	To	Lasting hours.
Aug. 19	7 P.M.	10	3	10	11	1	11	12	1	7 P.M.	12 P.M.	5
21	6 P.M.	8	2	8	10	2	10	11	1	6 P.M.	11 P.M.	5
23	5 P.M.	7	2	7	9	2	9	10	1	5 P.M.	10 P.M.	5

August 24. Six grains of quinine. 25th. Thirteen grains of quinine in three doses. Escaped the fit. No lip-eruption.

Attack 20. Eight months after, at Penetanguishene, there was aparoxysmal ague for two days, there being no distinct fit, or part of one.

April 18, 1844. Last night exposed to a cold draught, and today extreme weakness in the back and limbs, and particularly in the ankles, which symptoms continued the whole of the day, and also all the 19th.

April 20. Well. No lip-eruption. No medicine taken. There were here for two days the premonitory signs of a fit.

Attack 21. Five months after, (at Penetanguishene,) there was ague on the 30th October.

October 29. Got wet feet. 30th. At 6 P. M. languor, lassitude, anorexia, a feeling as if the ankle-joints were wanting, which sensations lasted all the evening.

October 31. At 8 A. M., the above premonitory feelings of a fit for seven hours, and at 3 P. M. a slight cold stage, which lasted four hours; then hot two hours, and sweating two hours. After the fit took eight grains of quinine in solution. On the next two days took eight grains more. No lip-eruption. This may be called checked tertian, as quinine was given before there was time for a second fit.

After this, for sixteen months up to March 1846, there has not been another attack of malarial fever. During this period he has been generally at Penetanguishene, along different parts of Lake Huron, up the river St Mary, at the Sault de Sainte Marie, and on Lake Superior.

Remarks.—In the twenty-one attacks there are seven tertians, four quotidians, four instances of monoparoxysmal ague, one of malarial continued fever, one of colic ague, two of incomplete, and two of aparoxysmal ague, (11 and 20.) A lip-eruption appeared in four instances, or a fifth of the whole. Two attacks began in January, two in February, three in March, four in April, one in May, one in June, two in July, two in August, one in September, two in October, and one in November. There was one attack in 1840, six in 1841, three in 1842, nine in 1843, and two in 1844. Fourteen were at Kingston, two in Grand River, one at Port Colborne, one in Edinburgh, one at Port Stanley, and two at Penetanguishene. There were eight attacks in winter.

The *causes* of the attacks may be referred to three heads. The first attack was from malaria alone. The 2d, 3d, 4th, 5th, 7th, 8th, 11th, 12th, 13th, 14th, 15th, 20th, and 21st, were from predisposition and exposure to cold and wet, or to cold alone. The 6th, 9th, 10th, 16th, 17th, 18th, and 19th, were from malaria and predisposition, and sometimes added to these were exposure to cold and wet. Generally speaking, no winter attack need be ascribed to malaria. All the places where they occurred are malarious, except Penetanguishene, which is very slightly so, and Edinburgh, which may be said not to be so at all. The symptoms were always noted down as soon after the fit as possible. During the fit there was not energy enough to observe the pulse. There is here omitted from the notes the mentioning of purgatives, which were always taken, and had acted before quinine was employed. With regard to the quantity of quinine taken, in one instance it is not noted, in five there was none taken, and in the remaining fifteen instances the average for each was twenty-three grains, the largest quantity being forty-five grains, (given in pills,) and the smallest twelve grains, (given in solution.)

Periodicity of the attacks.—In West India ague it was observed by Clark that a relapse sometimes took place on the 8th day in the quotidian, on the 14th or 15th in the tertian, and on the 21st or 22d day after the last fit in the quartan; and in the North American ague Jackson observed that the 14th, 16th, 8th, 12th, 20th, and 22d, were the most usual days, from which follows, as Dr Craigie observes,* the evident practical rule to give quinine till after the 22d day subsequent to the last fit.

Where a person after an attack of any disease recovers his usual health and strength, and then has a return of the same complaint, it seems more correct to call this a fresh attack than a relapse, and I shall now inquire if there be any regular periodicity in the attacks mentioned in case second.

Between the	No. of days of *interval*, or from the first day of previous attack to the first day of the present.	No. of days of *intermission*, or from the last fit of the previous attack to the first day of the present.
1st and 2d	210	177
2d and 3d	28	22
3d and 4th	24	18
4th and 5th	55	51
5th and 6th	148	140
6th and 7th	34	33
7th and 8th	47	44
8th and 9th	221	215
9th and 10th	41	37
10th and 11th	122	120
11th and 12th	31	25
12th and 13th	17	16
13th and 14th	11	10
14th and 15th	17	14
15th and 16th	20	17
16th and 17th	41	38
17th and 18th	29	28
18th and 19th	48	45
19th and 20th	243	239
20th and 21st	195	192

In reckoning the length of the intermission when there were no regular fits, the last day of illness is taken instead. Of the twenty-one attacks, six took place in or under the 22 days mentioned by Jackson and Clark; a tertian appeared 22 days after a quotidian; a tertian 18 days after another tertian, and a quotidian 14 days after a quotidian. In 4 years and 3 months there were twenty-one attacks, or one every two months and a half. The longest intermission is 8 months, and the shortest 10 days.

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* Practice of Physic, i. 12.

As far as this case goes, there does not appear to be any regular periodicity in the attacks of ague; they seem to arise not from habit partly, but from accidental causes alone. But on the other hand, the first case mentioned, that of Mr George S. seems to countenance the theory that habit has some influence in producing or re-exciting malarial fevers, and in similar cases the practical rule to be deduced is, where there is any doubt to give the patient the benefit of the doubt, and to advise removal to a healthy district some five or six weeks before the usual annual attack of fever, as by so doing not only will that year's attack be certainly escaped, but in after years, and without any such removal, the habitual attacks may perhaps be avoided.

Penetanguishene, *Lake Huron*,
March 1846.

Art. VI.—*An Experimental Inquiry into the Functions of the Ophthalmic Ganglion; with an application of the inferences obtained to the physiology of the Ganglionic System in general.*

By C. Radclyffe Hall, M. D., M. R. C. S. Eng., &c.

Part Second.

Influence of the sixth nerve on the iris.—That predaceous animals, by their habits constantly exposed to injury, should have correctness of vision insured by its non-connexion with alterations of common sensation, is nothing more than we should expect. Were the iris in the dog or cat under the direct control of the fifth nerve, a lacerated lip or bitten cheek would materially disorder sight. Why there should be a different arrangement in the rabbit and guinea-pig it is less easy to comprehend. These animals are fugacious, have their ears opening from behind, and can direct either eye by itself outwards and backwards, whilst the other is directed forwards, so as literally to look two ways at once. Is it not in order to associate the iris with the external rectus muscle, that the sixth nerve is distributed to both,—enabling the pupil of the everted eye to act consentaneously with the outer rectus whilst the pupil of the straight eye, in which the *rectus externus*, and consequently the sixth nerve, are at rest, is controlled at the time by its retina through the third nerve, in the ordinary manner? The fact that the pupils do not necessarily act together in these animals (§ 63), supports this view. It might appear easy to ascertain the truth by watching the eyes of a living rabbit; but from the dark colour and sluggish action of the iris, I have not been able to satisfy myself that abduction of the eye in the rabbit is invariably accompanied by contraction of the pupil.

Is there a motor nerve for causing dilatation of the pupil?—That the fifth nerve supplies no motor filaments for the dilatation of the pupil is proved by the experiments detailed. It has been conjectured that filaments proceed from the first cervico-spinal nerve, which, after passing through the superior cervical ganglion, become united with the sixth cerebral nerve, and ultimately reach the iris, over the dilating movement of which they preside. In support of this view, the principal facts adduced are, the contraction of the pupil, which results from removal of the superior cervical ganglion; the dilatation asserted to take place when that ganglion is irritated; and the following experiment of Guarini, "who having extirpated the superior cervical ganglion of an animal, and thus produced contraction of the pupil, administered a poisonous dose of strychnine (the effect of which generally is to dilate the pupil.) The pupil of the side operated on dilated but little, for the filaments of the third nerve now unopposed, maintained its contraction, whilst the pupil on the opposite side was enormously dilated, probably because of the radiated muscle (?) being put strongly into action by the stimulating effects of the strychnine on the spinal cord and so on the nerve (*i. e.* the cervical filaments inclosed with the sixth nerve) supplying this muscle."* Did the pupil dilate in obedience to a certain nerve, section of this nerve might cause momentary dilatation, just as we have seen that section of its contracting nerve causes momentary contraction; but on the subsidence of this effect of the immediate injury, we should expect *all* power of active dilatation, at least so far as the divided nerve was concerned, to be abolished. We should expect not that the pupil would dilate "but little," but that it would not dilate at all. Dr John Reid's experiments† are opposed to the assertion that irritation of the superior cervical ganglion causes dilatation of the pupil. In one experiment, on removing the lower half of the superior cervical ganglion in a dog, the pupil immediately contracted (Exp. III.) In a kitten, "when the sympathetic was compressed with a moderate force, the right pupil began to contract gradually, and became much smaller than that of the left eye; and it again resumed its former size on removing the pressure (Exp. IV.). If the compression in the latter experiment be considered to have paralyzed the assumed nerve of dilatation as it passed through the ganglion, the partial removal of the ganglion in the former must certainly have proved a mode of irritating any filaments connected with the unremoved portion of the ganglion. Yet, the effect on the pupil was the same. Guarini's experiment with strychnine is also invalidated by Dr J. Reid's observation, that on poisoning a dog with Prussic acid, both pupils became equally dilated, notwithstanding one had been previously contracted in consequence of the removal of a portion of the vagus and sympathetic. The influence of the sympathetic over the eye is not restricted to the iris. In the dog, experimental interference with the superior cervical ganglion, whether by section, compression, or complete removal, produces the same phenomena. Contraction of the pupil, retraction and inversion of the eyeball, partial closure of the eyelids, protrusion of the cartilaginous membrane, and increased vascularity of the conjunctiva, with more or less intolerance of light, result. (Dr J. Reid.) Of these effects, the retraction of the eyeball and the protrusion of the third eyclid, are due to excitement of the sixth nerve; the approximation of the eyelids, either to the same from retraction of the eyeball removing the usual support of the eye from the lids, or otherwise to excitement of the facial nerve causing contraction of the *orbicularis palpebrarum*; the inversion of the eye, to excitement of the third nerve acting upon the *rectus internus*. To ascribe the contraction of the pupil, therefore, to abolition of the function of the supposed dilating nerve, is to assume that the same injury of the superior cervical ganglion, which indirectly *excites* the function of several muscular nerves, *destroys* the power of one, viz. the nerve for dilating the pupil. In that case the power of dilating the pupil would be permanently lost, which is not the fact. If, on the other hand, we suppose it to be stimulated in common with the rest, the nerve assumed to be sent to the iris through the superior cervical ganglion must be one for contracting the pupil, the addition of which to the third nerve seems unnecessary. A poisonous dose of prussic acid dilates the pupils in all animals in which I have tried it. It can scarcely be supposed to stimulate the dilating, and at the same time to paralyze the contracting nerve. Destruction of the spinal chord produces maximum dilatation of the pupils (§ 38), which lasts for a considerable time. Assuming the existence of a spinal nerve for dilating the pupil, the destruction of the chord would cause the now unnopposed contracting nerve to lessen the pupil. And were dilatation of the pupil under the direct control of the chord through the medium of a spinal nerve, either enlargement of pupils should attend every instance of undue excitement of the spinal centre, which experience contradicts; or else the contracting force must greatly exceed the dilating, which is irreconcilable with the action of belladonna. We should also expect, as "the spinal chord never sleeps," that the state of the pupil during sleep, if referrible to nervous influence at all, would be that over which the spinal chord presided;—dilatation, according to the view in question; contraction, according to fact. In experiments I have never witnessed instantaneous enlargement of pupil which could not reasonably be accounted for by the suspension of the contracting influence of the third nerve; or which could be explained, without manifest error, on the supposition of direct irritation of any imagined nerve for dilatation. Pathology supports this opinion. The fixed expanded pupil in palsy restricted to the third nerve, without any deficiency of power in the fourth, sixth, seventh, eighth, or ninth cerebral nerves, or in any of the spinal nerves, proves that the *sphincter iridis* derives no motor power from any nerve except the third: whilst the activity of the pupil in hemiplegia, provided the third nerve remains unaffected, equally proves that dilatation of the pupil is not owing to, and dependent upon, any motor influence supplied by the spinal chord. Neither does anatomy afford any evidence of the existence of a special nerve for dilating the pupil. Observing the large supply and equable distribution of ciliary nerves to all parts of the iris, either the dilating nerve-filaments must be packed up with the rest, proceeding from the ophthalmic ganglion, which the uniform occurrence of contraction of the pupil on irritating these fasciculi forbids; or, otherwise, there should exist as obvious a supply of iridal nerves from some other source, as distinct in their course, and equable in their distribution as the ciliary nerves. These we do not find. From anatomical, as well as from physiological and pathological considerations, therefore, we have at present no reason to admit that there exists any special nerve, either encephalic or spinal, for dilating the pupil. I have elsewhere endeavoured to prove that there are not really any radiated muscular fibres in the iris, and consequently that dilatation of the pupil is not a muscular act.*

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* Ranking's Abstract, vol. ii. p. 316.

† Edinburgh Med. and Surg. Journal, No. 14.

Influence of the fifth nerve on the iris.—We have found that the fifth nerve is neither a motor, nor (as Valentin and others assume) an excitor nerve to the iris. Yet pathology is rich in cases inexplicable, unless we ascribe to the fifth nerve the power of affecting in some way the internal structures of the eye. Amaurosis from injury to the supraorbital nerve, or from the lodgment of a foreign body in the nostril; temporary blindness in severe facial neuralgia, in each case with corresponding alteration in the size of the pupil, illustrate this, and direct our attention to a fact which must be borne in mind in attempting to explain this subject,—viz. that whenever the size of the pupil is affected through the medium of the fifth nerve, there is invariably some modification of vision in that eye at the same time. The following cases are selected with reference to this question:—

Case 1. A dressmaker, aged 28, had a large, opaque, white cataract in the left eye, the result of a blow during childhood. Vision of the right eye perfect. Whilst both eyes were open the pupils were equal in size, and acted consentaneously. When the right eye was completely covered the woman could distinguish between a bright light and darkness with the cataractous eye, but could not discern the outline of any object. As she could see as well as feel brilliant light, it was evident that the retina was sound. On covering the right eye and directing a cone of concentrated solar rays through the pupil of the cataractous eye, the gaping pupil became slightly smaller, the iris acting in a sluggish manner, very different from its mode of action when the other eye also was exposed to light. Here the optic, fifth and third nerves, were all perfectly sound ; yet, as the cataract prevented all but very faint rays of light from reaching the retina, the optic nerve could be only slightly stimulated, and hence the resulting movement of the iris was slight. Sometime after the cataract had been removed, the iris of the left eye responded to light as quickly and fully as that of the right eye. But might not the excitor nerves in this case be retinal filaments of the fifth nerve, which, during the existence of the cataract, could be only feebly excited by the rays of light ? Such a conjecture is negatived by the next case.

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* Edinburgh Med. and Surg. Journal, 1844, p. 110.

Case 2. A shoemaker, aged 43. Perfect amaurosis of the left eye of three years' duration. Vision of the right eye perfect. Both eyes being open, the pupils were equal in size, and both irides acted alike,—proving that both third nerves were sound. When the right eye was perfectly closed the left pupil became largely dilated. Under these circumstances the concentrated rays of the sun were directed through the expanded pupil of the blind, left eye. There resulted no action of the iris, but in a short time a sense of uneasiness and warmth of the eyeball, an effort to wink, and slight lacrymation. The common sensibility of the parts was unimpaired, since the man could feel my finger when gently applied to the conjunctiva. Hence the fifth nerve was sound both on the exterior, and in the interior of the eye. The lacrymation proves stimulation of the lacrymal nerves ; the effort to wink, stimulation of the facial nerve, both probably excited by the sensational portion of the fifth nerve. The immobility of the iris, although its motor nerve had been proved sound and capable, proves that stimulation of the fifth nerve sufficient to react on other motor nerves, excites no action in those of the iris.

These cases appear to prove that the optic is the only excitor nerve, and the third the only motor nerve, through which contraction of the sphincter pupillæ is produced : that the fifth nerve may be stimulated so as to induce closure of the eyelid in the excito-motory manner, and yet fail to induce action in the iris. To complete the pathological evidence, we should ascertain whether there can be perfect excito-motory action of the iris, although the fifth nerve has lost its power. There are cases of anæsthesia of the fifth nerve recorded, in which the iris is stated to have acted normally. I long searched attentively for such a case, when hospital practice furnished an extensive field for the purpose; but amongst many cases of anæsthesia of the fifth I never met with one in which the iris acted quite normally. The action appeared to be normal in some instances, so long as both eyes were open, but not after covering the eye on the sound side. In every case the visual power on the side affected was always more or less impaired; and the degree of impairment was the measure of diminished activity in the iris.

Case 3. A milliner, aged 40, a delicate and intelligent woman, for four years had been subject to occasional attacks of severe facial neuralgia. The paroxysms commenced suddenly with acute darting pains of the left cheek and forehead, lacrymation, dimness of sight and rapid winking, on the left side. In a few minutes the left eye lost all power of vision, so that on covering the other the woman could discern nothing. *Whilst the right eye was thus covered, the pupil of the left was largely dilated.* During the paroxysm the left side of the face was more flushed and hotter than the other, and until the anguish caused a flood of tears, the weeping was almost restricted to the left eye. The ordinary duration of a paroxysm was half an hour. On its subsidence, the power of sight gradually returned to the left eye. In the intervals between the paroxysms there was no deficiency of visual or of common sensibility, or of activity in the iris; hence the optic, fifth and third nerves were essentially sound. The motionless and dilated pupil of the left eye, at a time when the left fifth nerve was greatly stimulated, provided consent with the sound eye was prevented, proves that excitement of the fifth nerve does not necessarily cause contraction of the pupil. The agreement in size of the pupils when both eyes were open equally proves that excitement of the fifth nerve, if it lead to dilatation of the pupil, does not cause this by antagonising and overpowering the third nerve. On what, then, did the modified activity of the iris depend in this instance? The blindness proved that the visual apparatus was deranged; the dilated pupil was merely the outward sign of this derangement. Having seen that when visual sensibility is lost, the fifth nerve cannot affect the pupil,—that when visual sensibility exists the fifth nerve can affect the pupil,—and knowing that visual sensibility governs the normal action of the iris, we must infer that whenever the size of the pupil is influenced by the fifth nerve, it is so only through some intermediate affection of visual power. The question then arises, is the influence of the fifth nerve at all times essential to the maintenance of vision, or has the fifth nerve merely the power of influencing in some way the sentient parts of the apparatus of vision, so as to modify or prevent their normal action? Besides the *a priori* improbability of one nerve depending, for the ordinary performance of its function, on the assistance derived from another, a single authentic case of complete insensibility of the fifth nerve, and yet with perfect vision of the eye on the same side, would completely negative the first question.

How can the fifth nerve affect the sense of sight? We may presume in several ways. (1.) When stimulated, the fifth nerve might communicate its excitement to the centric part of the optic tract, and thus heighten the acuteness of vision. (2.) By exciting the grey cells of the Gasserian ganglion, it might cause ganglionic influence to pass along grey fibres to the lenticular ganglion and thence to the vascular coat of the retina. (3.) The fifth nerve might itself excite the grey cells of the lenticular ganglion, and thus react on the retina through the ganglionic nerves.

In the first mode, the fifth nerve could cause contraction of the pupil only; whereas under extreme pain, it sometimes induces dilatation. Is this dilatation owing to incapacity of the perceptive part of the nervous apparatus of vision, from such an amount of cerebral disorder having been induced as to render the sensorium unable to perceive visual impressions, although duly conveyed to the brain? In that case, as the fifth nerve is not more intimately connected at its centric termination with the optic than with the other nerves of special sense, we should expect deafness as well as blindness; loss of taste, if not of smell. These are not met with. Nor, if its impairment depended on cerebral disturbance, should we expect the sense of sight to be so rapidly restored on the subsidence of the irritation of the fifth nerve as it really is. We must consequently rather assume one or both of the two last modes laid down as those by which the fifth nerve reacts on the iris. Both equally require us to admit that cerebro-spinal nerve-tubes have the power to affect the ganglionic grey vesicles through which they pass; and the third mode requires us also to admit centrifugal action of sensational nerve-tubes; since the only fibres of the fifth which enter into the lenticular ganglion are traceable from it backwards into the Gasserian ganglion, and therefore could communicate no influence to the former, but what they had already conducted from the latter, *i. e.* excentrically. How can we otherwise account for any branch of the fifth nerve passing into—and through the lenticular ganglion at all? It is not required for the purpose of endowing the eyeball with sensibility, since the fifth nerve furnishes for that purpose a larger supply which is quite unconnected with the ophthalmic ganglion. It is not to confer motor power on the ganglion, since the third nerve supplies of itself, irrespective of the ganglion, all the motor nervous power which we can demonstrate in the eye. It cannot be merely to serve as a conductor of organic nerve-filaments from the sympathetic, for these might just as conveniently have been packed up with the third nerve, as some usually are. It is not needed to connect the retina with the facial nerve, in order that a dazzling light may excite closure of the eyelids, since the naso-ciliary nerves, which do not enter the ganglion, might have affected that. What improbability is there in conceiving that one purpose subserved by the peculiar relative position of the vascular coat of the retina, placed as it is in front of the retiform nervous expansion of the optic nerve, and amongst the minute papillæ which doubtless constitute the real sentient field of vision, is that of modifying the effects of stimulation according to ever-varying conditions of degree and quality of light and heat? It can scarcely be doubted but that mere excited circulation through this vascular coat, within limits, would heighten; tardy circulation diminish; and a congestive state greatly impair, the sensibility of the retina. As a mere hypothesis, therefore, it is allowable to conjecture that the small supply of fifth nerve which enters into the lenticular ganglion, passes through to the retina exclusively; that when in the ganglion, it can affect, during its own conduction of nervous force, the action of the ganglionic vesicles, and consequently call forth the influence, be that what it may, of the ganglionic nerves. Begging the question, for the moment, that one effect of the ganglionic influence is the afflux of blood to the part, we can understand how the stimulation of the retina by intense light may cause not only instantaneous contraction of the pupil through the third nerve, but also an altered state of circulation through the retina, through the medium of the fifth nerve, lenticular ganglion and ganglionic nerves, producing painful confusion of sight, or, in other words, dazzling and aching in the eye. The unusual supply of blood furnishing at the same time the means of recruiting the over-excited retina. We can understand how one amount of stimulation of the fifth nerve may have such a slight effect on the vascular coat as may merely stimulate the retina, and thus cause contracted pupil (*e. g.* ulcer of cornea, strumous ophthalmia): how a greater amount of stimulation suddenly applied, or the same amount greatly prolonged, may cause so much disorder of the retinal capillaries, as to produce temporary blindness and dilated pupil (*e. g.* severe *tic*). The fifth nerve being in the one case indirectly an excitor of contraction; in the other, of dilatation of the pupil.

If the nerve-tubes which pass through the ophthalmic ganglion have the power of exciting the ganglionic grey cells and their organic nerves, the third nerve must act in this way every time that it causes contraction of the pupil. Why then does it not likewise produce a similar effect to the fifth on the vascularity of the retina? Such an association would have interfered with correctness of sight; since when vision was required to be very minute and exact, as in examining a small and near object, the greatly contracted pupil would have had as its concomitant a gorged state of the blood-vessels of the retina. Still it is most probable that the third nerve, as well as the fifth, does affect the state of capillary circulation within the eyeball, and influences, through the medium of the lenticular ganglion, a still greater extent of vascular apparatus than that of the retina, viz. the capillary plexuses of the ciliary processes and of the iris, subservient as these probably are to focal adjustment, which is always attended with alteration of pupil. That the filaments of the fifth nerve, and those of the third, should excite different parts of the lenticular ganglion,—if they do so,—is quite conformable to what the physiology of all portions of the nervous system tends to confirm, that speciality of action depends solely on the centric connection and ultimate distribution of the nerve-tubes. Anatomy furnishes only the negative support of supplying no facts irreconcilable with this view. Nor can it fairly be objected that it ought to do more; since the only positive evidence would consist in our being able to follow an identical nerve-filament from the encephalon through the ophthalmic ganglion, noting its relations there, tracing it on into the eye, and ascertaining the exact point and mode of its final termination. Those most accustomed to difficult dissections will be the first to acknowledge the impossibility of doing this. Unless it be in order to associate them together in their action, there seems no reason why the sensational, motory, and organic nerves of the interior of the eye should be so intimately connected together in the form of the lenticular ganglion before proceeding to their destination. The excito-motory action of the iris takes place independently of the ganglion, has its nervous centre elsewhere, and therefore could have been effected without it. The fifth nerve does convey most of the common impressions, and therefore could have conveyed all without entering the ophthalmic ganglion. The sympathetic could have furnished directly the organic fibres. If we imagine an animal so placed that the medium in which it lives forbids any sudden or great variation of light from over-stimulating the retina, and therefore the focal adjustment of the eye, and the special sensibility of the retina, could not with advantage be placed under the influence of the nerve of common sensation,—in such an animal, there would be nothing gained by the ocular twigs of the fifth nerve passing in close relation to the ganglionic cells and nerves. If, again, the iris were nothing but a mere immovable, non-muscular septum, the pupil never varying in its size, there would be no reason for associating the ocular twigs of the third nerve with the ganglionic. And as neither the sensational nor the motor nerve required connection with the organic, the interior of the eye might derive its ganglionie supply directly from the sympathetic. This combination of conditions, imagined as theoretically probable, is precisely what we meet with in the fish.*

In birds and most mammals there is a movable iris; and wherever this exists there is always an ophthalmic ganglion. The constancy of this co-existence points out a functional relationship; and were the iris merely a muscle in structure and action, it would favour the supposition that the ganglion is for the purpose of presiding over its muscular action. But the *sphincter iridis* can never act without at the same time affecting the extensive vascular apparatus so intimately connected with it. Finding then, that when there is vascular structure without muscle, the ganglionic nerves are not connected with the motor ones; where there is vascular structure so connected with muscle that the two always act consentaneously, the ganglionic nerves are intimately connected with the motor ones; the conclusion is obvious, that such anatomical connection is for the purpose of permitting of functional association. Of what nature is this functional association of the three (physiologically) different nervous elements of the lenticular ganglion? As we have found that the fifth nerve does not act directly upon the third, nor the third nerve upon the fifth, through the medium of the ganglion, only two kinds of combination of function remain.

(1.) The action of the nerve-tubes transmitted, both sensational and motor, stimulates and calls forth the action of the ganglionic nerve-vesicles, amongst which they pass, and thus excites the organic nerves which proceed from these vesicles.

(2.) The ganglionic nerve-vesicles being themselves in action in the first instance, excite secondarily the action of the transmitted nerve-tubes.

Were both suppositions correct, we should never have contraction of the pupil without sensation, nor without action of the ophthalmic ganglion. Were the second true, we should never have action of the ganglion, assuming that the ganglion furnishes no check to the transmission of sensation, without contraction of the pupil and sensation as its attendants. We are not conscious of the changes of the iris in exercising sight, and therefore we can have no common sensation implicated. The first supposition consequently appears to be the true one. Provided that no error exist in the facts, and no unreasonableness in the considerations, which have been stated, as we cannot suppose the arrangement of the ophthalmic ganglion to be peculiar and distinct from that of other ganglia, we may educe two general laws;—

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* The function of the third nerve as regards the interior of the eye of the fish, judging from its distribution, and from the non-existence of any muscular tissue within the eye, is that of presiding over the fulness of the vessels of the choroid body, on which it is presumable the adjustment of vision for different distances depends. If so, the third nerve in the fish, as in higher animals, acts responsively to the optic nerve. As there is no lenticular ganglion, this would support the conclusion that the ganglion is not a centre of excito-motory action. It would also be an argument, as far as it went, that a nerve usually muscular can act on other structures if so distributed.

(1.) That when encephalic, spinal, or cerebro-spinal nerves pass through the substance of a ganglion, it is in order to associate the influence of the organic nerves of that ganglion with their own.

(2.) But that, on the other hand, during ordinary excitement, the ganglionic nerve-vesicles have not the power of exciting to action, the white nerve-tubes, which lie amongst them, in their passage through the ganglion.

In the preceding remarks, several questionable points in the physiology of the nervous genglia in general, have been referred to, which it is desirable now briefly to examine.

Do the ganglia impede sensation? The sympathetic ganglia have been supposed to check the sensations which, it is presumed, would otherwise be conveyed from the viscera. In perfect health, the only feeling experienced from the viscera is the obscure sense of general comfort,—that surest sign that the vital organs are acting in harmony. But let the action become disordered, as in spasm of any part of the alimentary canal, and there is extremely acute sensation. This sufficiently proves that the ganglia do not always impede sensation. Nor, in the absence of proof, is it necessary to assume that they ever do so. A ligament communicates no marked sensation during the ordinary movement of a joint, but let it be stretched suddenly, and severe pain results. Here we have no ganglion implicated but the spinal one; and, that spinal ganglia do not interrupt sensational action, is certain from their position on the posterior spinal nerve-roots. In both ligament and viscera the sensational nerves are so arranged at their distributed extremities as not to receive any stimulus except under unusual and irregular conditions; but when from disordered action of the part they are stimulated, they communicate their excitement without any interruption from the ganglia. We cannot suppose that the Gasserian and the spinal ganglia impede sensation; and there is no anatomical peculiarity about the ganglia of the sympathetic nerves to sanction our adding this to their endowments. The hypothesis that every ganglion lessens the sensibility of the sensational nerves which it transmits must therefore be considered as devoid of proof, against analogy, and unnecessary as well as insufficient for the explanation of the phenomena on which it was founded.

The diametrically opposite opinion, *that the ganglia are essential to sensation*, has greater plausibility from the circumstance that every sensational nerve is a gangliated nerve also. It is disproved by the fact that the posterior roots of the spinal nerves are sensitive before they enter the ganglia.

Do the ganglia prevent the transmission of the influence of the will? The motor nerves of voluntary muscles never pass through ganglia; those of involuntary muscles invariably do so. Hence the ganglia have been thought to obstruct the transmission of volitional power, and until Dr Marshall Hall had established the independency of the spinal chord, this theory was not unreasonable. Now, however, that the entire class of involuntary external excito-motory acts are admitted to be performed by spinal muscular nerves which are not gangliated, the assumption is not required for the explanation of involuntary motion. Every involuntary muscle is either directly or indirectly connected with the spinal chord, and such connection is sufficient to account for all involuntary excited muscular action. The third nerve causes the *sphincter iridis* to contract, as we have seen, without sustaining any obstruction from its iridal filaments passing through the ophthalmic ganglion. By experiment we find that, through the medium of the spinal chord, we can excite, diminish, or destroy the contractility of involuntary muscles instantaneously, and as the chord can influence these muscles only through the medium of nerves which pass through sympathetic ganglia, it is evident that these ganglia offer no impediment to the transmission of spinal *vis nervosa*. As we have every reason to believe that the mechanism of nervous action is identical, whether the impulse to the action originate in the brain or not, it is a fair inference from analogy that the sympathetic ganglia offer no obstacle to the transmission of cerebral force. The viscera, though removed from the direct influence of the will, are greatly affected by mental emotion, which may in some measure be controlled by the will. We may conceive that the visceral nerves are connected in the encephalon with the centre of emotion, but not directly with that of volition. That certain nerve tubes have this arrangement anatomical proof will hereafter be furnished. That some of these nerve tubes have the destination above assigned, is, and probably must remain, purely a conjecture.

Do the ganglia supply motor nervous energy to the involuntary muscles?

Experiment XXIII. A small dose of prussic acid was given to a rabbit. The animal shrieked, was convulsed for thirty seconds, and then died.

122. During the convulsive action of the muscles, the pupils were first contracted, then dilated; in two minutes after death they began to contract again, and afterwards remained of less than the medium size.

123. The integuments were stripped off the abdomen without tearing the transparent parietal layer of peritoneum. Stomach distended with food. Intestines motionless.

124. On opening the chest and noticing the heart, the auricles were found to be contracting rythmically; the ventricles distended and motionless. Both auricles contracted together, then occurred a pause for two-thirds, then a slight but uniform contraction for one-third of the period. As nearly as could be ascertained, this regard to time was observed until the action of the auricles ceased entirely; the cessation being preceded not by the intervals becoming longer, but by every contraction becoming more and more feeble. On touching either of the ventricles with the scalpel point, both instantly responded by a tucking upwards of the apex of the heart; the muscular action not appearing to spread gradually from the spot irritated, but to affect instantly a distant part, viz. the apex. This contractility of the ventricles ceased in about five minutes; but after this, on pressing out part of the blood with the finger, the ventricles would for a moment afterwards respond to irritation. Hence their contractility was impaired by over-distension as well as by exhaustion.

125. Whilst the irritability of the heart yet remained the peritoneum was opened, the stomach lifted up, and the semilunar ganglia touched with the scalpel. Active vermicular motion of the small intestines immediately ensued, and continued for a short time after I had ceased to irritate the ganglia, but after quiescence it instantly recurred on renewing the irritation of either the ganglia or of the principal nerves issuing from them. When the action of the heart had entirely ceased, the intestines no longer responded to irritation of their ganglia, but as the heart could scarcely have ministered to the circulation previously, this may have been a mere coincidence, arising from the parts having been for some time exposed, stretched out, and cooled, and the nerves having perhaps become exhausted by the irritation, simultaneously with the heart's loss of irritability. After the bowel had ceased to respond to irritation of the nerves, slight muscular contraction could be induced by scratching its coats.

126. Scratching the grey ganglionic mass about the origin of the renal artery had no apparent effect at any period of the experiment.

Experiment XXIV.—A full grown rabbit. Exposed the peritoneum so as to see the viscera through it. The animal gave no sign of pain.

127. The stomach and intestines were motionless. On making a small opening in the peritoneum some of the small intestines protruded, and the instant they were exposed to the air vermicular motion commenced

128. A drachm of prussic acid was dropped amongst the intestines. This appeared to stimulate the muscular coat of the bowels to which it was applied; the peristaltic action being increased.

129. In twenty seconds the rabbit shrieked and was convulsed; the stomach and all the intestines were suddenly and forcibly protruded through the small aperture in the peritoneum; the action of the heart became weak and fluttering, and ceased in a minute and half.

130. The pupils at first contracted to their utmost, then dilated, until when the heart cased to beat they were at their maximum of dilatation. Not observed subsequently.

131. The peritoneum was slit up. Stomach and duodenum were distended with food. These distended parts were motionless. The remainder of the small intestines moved gently. On exposing the semilunar ganglia, and scratching the greyer portion, peristaltic action was instantly increased; but was still more suddenly and vigorously increased by irritation of the whiter portion, or of the nerves proceeding from the ganglia.

132. Some influence over the motion of the intestines was likewise manifested, but not to so marked an extent on irritating the renal plexus.

133. The blood-vessels of the viscera appeared to be full; and the arteries did not empty themselves after death. The heart also was full in every cavity.*

In these and similar experiments we have no proof that the ganglia themselves generate the motor nervous force which, under irritation, they impart to the muscular coat of the alimentary canal, since there is no ganglion that does not contain and transmit spinal nerve-tubes; and the contraction excited in the muscle is always considerable in proportion to the quantity of white matter, *i. e.* the number of spinal and encephalic nerve-tubes in the ganglion. If the ganglion is very grey and translucent it is difficult to excite any muscular contraction by irritating the ganglion; but if white and dense, (*e. g.* the cœliac) then contraction readily ensues, but never more readily nor more extensively than when the irritation is applied spinad or peripherad to the ganglion. We have therefore no proof that any ganglion either itself generates nervous force for muscles, or that it in any way affects the force conveyed by the spinal nerve-tubes which pass through it. Whilst the contrary is proved with respect to the lenticular ganglion. To consider the ganglia centres of nervous energy for muscles appears superfluous when the spinal chord exists as a distinct centre for that express purpose. And if the ganglia were intended in any case to supersede the chord, why should we have spinal nerves passing through every ganglion? A principal reason perhaps for assigning the functions of the chord to each ganglion, is the fact of many active but simply organised animals possessing ganglia as their only nervous centres. Here undoubtedly a ganglion acts as an excito-motory centre, and probably serves the purpose of sympathetic and spinal centre at the same time. But in asterias each of the five ganglia has an equal right to be considered the sensorium; and were a loose analogy to guide us, we should therefore revert to Winslow's opinion, and consider every ganglion in higher animals a "lesser brain!" The very fact of separation as we ascend the scale is an argument against perfect similarity of function. Dr Carpenter has shown us how to deduce important inferences from the study of the nervous system of invertebrata.* To his conclusions the following may perhaps be added as having reference to the present question.

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* By way of caution to others, I may state that I suffered considerably from carelessness in the use of prussic acid in conducting this experiment. Whilst intently observing the points above noted, my face was held very close to, and over, the parts, and was of course exposed to the warm exhalation conveying the vapour of prussic acid from the viscera. Eight or ten minutes had elapsed before I experienced any thing unusual, when suddenly dimness of sight, vertigo, and a peculiar feeling of expansion, and lightness of the chest, and want of breath occurred. On leaving the room, which of course I did instantly, I found I had scarcely any muscular power. Stimulants, cold water, and fresh air were freely used, but the uncomfortable effects lasted for several hours.

1. When different parts of an animal have similar functions to perform, but at the same time it is necessary that each part shall be capable of acting by itself, independently of the rest, each part thus functionally independent possesses a nervous centre of its own, *e. g.* Talytrus; Scolopendra; larva state of insects.

2. But when several parts not merely act in a similar way, but always act simultaneously, one nervous centre is common to all the parts thus functionally combined, *e. g.* the amalgamation of ganglia in various mollusca, (Limax; Pecten;) and in the imago state of insects.

3. When the habits of the animal are such that vision, common sensation, and muscular action need never be manifested singly or separately, one nervous centre is subservient and common to all these functions, *e. g.* Echinus; Asterias.

4. But as we rise in the scale, and the circumstances of the animal require the ability to perform each of its more important and varied functions independently of the others, each different set of organs thus functionally individualized has its distinct nervous centre, *e. g.* Sepia; Patella; and the higher forms of all entomoid articulata.

5. Where the nervous centres preside over movements which usually and normally are performed in concert, there is a direct commissural connection between them, allowing excitement in one to be readily communicated to the rest, *e. g.* Articulata in general.

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* Inaugural Dissertation, Prize Thesis, 1839.

6. But where such co-ordination does not exist, and would defeat the end, we find no such commissural connection, *e. g.* the non-connection of the pharyngeal with the pedal and palleal ganglia in several mollusca; also, that of the ganglia on the so-called recurrent nerve with the ganglia of locomotion in various insects.

7. The ganglia are not developed either in number or size in proportion to the sensibility of a part; but they are so, in proportion to the amount of independent muscular actions which that part was to perform, *e.g.* compare different Myriapoda, Crustacea, and the several stages of insect life.

8. Where sensorial control is exercised, nerve-tubes pass to the organs already supplied with nerves by their own nervous centres. These sensorial nerve-tubes form a tract distinct in its entire course from the nerve-centres over which it passes, *e. g.* Myriapoda, Crustacea, Cephalopoda, the higher insects in their perfect state.

9. From this sensorial tract some fibres pass into (through ?) each ganglion, thus associating the force of the ganglion with that of the sensorium in every volitional movement. Other fibres pass from the sensorial tract without being in any way connected with the ganglia. Are these true sensational fibres ? *e. g.* the same as the preceding.[*]

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[*] In various Articulata there are three tracts discernible in each longitudinal chain of ganglia. The highest and smallest fibrous column proceeds from the supra-œsophageal ganglion over, and closely attached to the subjacent gangliated tract for its entire course, but does not enter into the ganglia. Assuming that the supra-œsophageal ganglion is the sensorium, this might be considered the sensorial tract. The two remaining tracts are amalgamated inseparably, and both enter into the formation of the ganglia, but the higher portion appears to contain fibres continuous with the supra-œsophageal ganglion; the lower portion merely fibres which pass from ganglion to ganglion, and which never reach the supra-œsophageal. Of the two lower tracts, therefore, the higher portion may be distinguished as the gangliated sensorial tract, the lower as the ganglionic tract. Each compound nerve has three roots, corresponding to, and derived from these three tracts; a large root containing fibres from the two lowest tracts, proceeding directly from the ganglion; and a smaller root from the highest tract, which has no connection with the ganglion at all, but joins the rest of the nerve separately. This triply-compound nerve is distributed to sensitive parts, and to muscles which act both voluntarily and involuntarily.

Physiologists are agreed that the involuntary action, so far as it implicates the nervous system at all, is due to the ganglionic fibres alone; but with respect to the function of the two remaining tracts, viz. the gangliated sensorial, and the non-gangliated sensorial, there is difference of opinion. Dr Grant, who first directed attention to it in this country, Mr Newport, and Dr Marshall Hall, appear to view the highest fibrous column as motor only; and the next or gangliated tract, either as moto-sensitive (Grant), or as sensitive only (Newport) Dr Carpenter deems it more probable that the non-gangliated fibrous tract is both sensory and motory; whilst Todd and Bowman, not satisfied that we have yet ascertained the exact anatomy on this question, seem to consider all the fibres as commissural only. The last view leaves the existence of the peculiar arrangement of these several tracts quite unintelligible. As no instance has been made out in which fibres pass directly from one of these tracts to their destination without being previously mixed up with fibres from the others, we have no means of ascertaining by the distribution of the nerves the functions of their source; and as

10. Where sensorial influence, if greatly exercised, would interfere with the normal action of the ganglion inconveniently, such distribution of sensorial nerve-tubes is very small, *e. g.* the slight connection of the ganglia which supply the respiratory organs with the supra-œsophagal ganglion in insects.

11. The sensorial centre is that with which the nerves of the special senses are most directly connected. In proportion to its inferiority in size, are, judging from the habits of the animal, the volitional acts mere responses to sensations; the magnitude of a nervous centre being an exact criterion of its power; *e. g.* compare the imago with the larva state of insects; Julus with Scolopendra; the scorpion with the spider.

12. The concentration of the nerves appropriated to several dissimilar functions in one ganglion is a mark of inferiority in the scale of animals, indicating a small amount of independency in the several functions. On the other hand, the concentration in

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the tracts are too minute to permit of experimental procedure, any attempt at explanation must be merely hypothetical. This being the case it is less presumptuous to dissent from the opinions of the above-named physiologists. It seems to myself most probable that the *non-gangliated* sensorial tract is sensiferous only, and that it has no connection with the ganglion in order to permit a sensational impression to affect the sensorium without of necessity calling forth ganglionic action; that the *gangliated* sensorial tract is the volitional motor column, and that its fibres *do* pass through each ganglion in order to excite its co-operation, since voluntary muscular movement is for the most part effected by the same muscles (as also in higher animals) as that which is produced in the excito-motory manner; and that both sensorial tracts arise in a centre separate and distinct from the chain of ganglia, in order that each ganglion may have the power of acting by itself, independently of sensorial influence (*i. e.* physically). Where sensorial influence is so little required, that it does not interfere with, and is possibly never manifested without the exercise of the other nervous functions, there has yet been discovered no such separation of nervous tracts (*e. g.* Echinodermata). In the arms of the octopus, Dr Sharpey discovered a double nervous column, one tract gangliated, the other not. The echinus fastens itself by means of tubular suckers, just as the octopus does, but it is sluggish, its hard exterior little sensitive, and all its powers comparatively feeble. Hence it has small demand for sensorial action. Whereas the octopus is active, energetic, and has probably very delicate sensibility in the surface of its pliant tentacula. Coexistant with superiority of sensibility and of sensorial power, therefore, we find the distinct *non-gangliated* chord. It is no real objection that in vertebrata the sensational nerve-tubes invariably do pass through ganglia, whilst the volitional motor nerve-tubes as constantly do not. If we consider the ganglia of invertebrata as so many isolated segments of spinal chord, I believe the analogy will prove exact. Nor is there any argument against this to be founded on apparent difference of relative position of the several columns in the two great divisions of the animal kingdom. The chord in vertebrata commences and continues dorsal; in invertebrata it commences dorsal (in the supra-œsophageal ganglion), but afterwards becomes ventral. In bending down to the ventral surface, that aspect of the ganglionic chain which was at first nearest to the surface—dorsad, is now nearest to the viscera and v. v. Hence the highest of the columns in the ventral chain is the analogue of the posterior strand of the spinal chord. This, according to Sir C. Bell's views, would countenance the function I have assigned to it—that of sensation. But in vertebrata the most posterior strands of the spinal chord are probably not sensational at all; still, as the sensational strands of the chord are situated posteriorly in reference to the volitional and true spinal nerve-tubes, the analogy as regards relative position is exact also.

one centre of nerves appropriated to perfectly similar functions and the conjoint existence of such a centre for each important class of organs indicates superiority, admitting of greater extent and association of similar actions, and of greater independency of dissimilar ones; *e. g.* compare the lower with the higher species in each class of invertebrata.

If these remarks be admitted, it results that the separation of the nervous matter into distinct centres is for the purpose of individualizing the action of each centre, but not of investing it with any distinct and peculiar kind of power; that its amalgamation into fewer centres is to ensure greater association, and to diminish independency in the action of the parts supplied; that in all the higher invertebrata the exercise of sensation and volition is restricted to one nervous centre, which is connected with the rest in exact proportion to the degree in which the sensorial functions are implicated; that the variety in the results of nervous action is due solely to the centric connection and peripheral distribution of the nerves, and not to any intrinsic difference in the nature of the change which takes place in the nerve-vesicles. This is obviously begging the question of the similarity of physical action between the supra-œsophageal (sensorial) and the other ganglia, but we could hardly explain otherwise the combination of all the nervous functions in one centre which we meet with in several of the lower animals.

As the ganglia of invertebrata are developed in number and in size in exact accordance with the muscular powers of the parts supplied, we should have here an argument in support of the visceral ganglia in higher animals being nervous centres of muscular action. Most physiologists, however, coincide with Beclard, Muller, and Carpenter, in concluding that there is no analogue to a distinct sympathetic nervous system in the invertebrata. Traces of that anatomical separation of centre, which is the only point of resemblance, are met with in the lowest vertebrate animals, in which muscular movement is still the result more of physical than of sensorial action. Hence the variation in number and magnitude of the encephalic lobes in fishes, according to the degree of intelligence possessed. Where intelligence is lowest and the true cerebrum deficient, there is no sympathetic system whatever distinct from the rest, (Cyclostomi). Where the median lobes are large, the cerebrum very small, there first appears a rudimentary visceral nervous system, but still the *par vagum* supplies many organs in place of the sympathetic, (most osseous fishes and serpents); in proportion as the median lobes are less, the cerebrum larger, we find the *par vagum* less, and the sympathetic more developed, (sharks and other cartilaginous fishes). Continuing the investigation through reptiles, birds, mammals, up to man, we find the rule universal, that the sympathetic nervous system is developed in direct ratio, not with the nutritive powers, not with extent of sensation, not with muscular energy, but with the amount of intelligence of the animal: and consequently in inverse ratio to the magnitude of the *medulla oblongata* and *corpora quadrigemina*; in direct ratio to that of the cerebral hemispheres and cerebellum. It was not, therefore, without some appearance of probability that the older writers referred certain psychical operations to the action of the visceral ganglia. The inference previously stated, however, that anatomical separation is to ensure functional individuality, leads us to the diametrically opposite conclusion; and we consider the separation of the sympathetic ganglia from the other nervous centres as the means of removing their action from interference on the part of the mind. An interference prejudicial in proportion to the degree in which the unequal irregular influence of mental action could operate, and an interference, therefore, provided against in exact proportion to the amount of intelligence. As comparative anatomy furnishes no argument in favour of the ganglia in vertebrata being centres of muscular action, as another centre exists which is proved to preside over excito-motory action, and as the lenticular ganglion is proved not to preside over the muscular action of the *sphincter iridis*, in the absence of direct proof to the contrary, we may conclude that the spinal and prævertebral ganglia do not supply nervous influence to muscles.

Real Functions of the Ganglia.—Since the time of Bichat, the idea has been prevalent that, in some way not clearly defined, the ganglia governed the functions of organic life. By many, the sympathetic nerve has been considered the factor-general of all the vital operations; an hypothesis of which it is the sole merit that it renders the action of all subordinate agents abundantly simple. Modern researches have proved that every proximate constituent of a living body contains, complete within itself, the means of performing its specific action, so long as the requisite conditions are observed. The most important of these conditions is the due supply of organizable material. To regulate the supply according to varying demands, and to ensure order in the discharge of complicated functions which are at once mutually related and yet entirely different, there must be some means of harmonizing various organic actions, and regulating the supply of blood according to the requirements of the different organs. No other system except the nervous presents the characters fitted for effecting this superintendence. Many conjectures have been formed as to the mode in which the nervous system regulates vascular action. Dr Alison has pointed out that no modification of the heart's action, or of the contractility of the arteries, singly or conjoined, can account for all the phenomena of the capillary circulation in inflammation. The subject, indeed, has been greatly mystified by an attempt to reduce it to a mechanical explanation, though as little susceptible of it as the process of which this is the abnormal form —nutrition. Of the cause of the afflux of blood in an inflamed part, three hypotheses are offered. 1. Increased vital affinity between the particles of the blood themselves, and between them and the surrounding tissues, producing secondarily dilatation of the vessels. (Alison.) 2. Suspension of the nervous influence by which the small blood-vessels are normally maintained in a state of tonic contraction; consequent relaxation with dilatation of the vessels and retardation of the flow of blood through them. (Henle; Billing.) In addition to this condition, and likewise consequent on the suspension of nervous influence on which it is dependent, the normal repulsion of the red corpuscles of the blood is lessened, and their closer aggregation, and consequent stagnation of blood, permitted. (Wharton Jones.) 3. Active dilatation of the blood-vessels, (John Hunter); from increase in the nervous energy supplied, (Macartney); that supply furnished by the organic nerves, (Copland.)

Without attempting to discuss so extensive and important a question as that of the pathological character of inflammation, we must find the best clue to the normal influence of the nervous system on the blood-vessels; in the exaggerated manifestation of that influence in disease. A nervous system is not essential to either normal or abnormal nutritive secretion, for we see both in vegetables. Abstractly considered, therefore, the local changes of inflammation might possibly occur without any implication of the nervous system at all. But in higher animals, at least, we never have these local changes without concomitant affection of other parts, however slight the extent or degree. To this association of action innervation must be deemed essential. But the nervous system does more than this. It is admitted that in some way the nervous system can influence the action of the capillaries, but the mode by which this influence operates is not ascertained. Those who explain the first stage of congestion in an inflamed part as the result of a suspension of the nervous energy having led to a passive enlargement of the blood-vessels, are obliged to assume that the capillaries possess contractile walls; that their contractility in health is maintained by a constant supply of nervous energy; that prior and leading to congestion there is palsy of the vascular nerves. From which it would follow, that when the blood-vessels are contracted and contain the least blood the vascular nerves are in a state of over-action, but when the small vessels are filled and distended, their nerves are temporarily palsied. Whilst digesting a meal the mucous membrane of the stomach is most vascular; its organic nerves must then be least active! Scratch the pale skin on the back of the hand, there is first a white line from the compression, then sensation, increased redness, and perhaps bleeding. The increased redness proves distension of the capillaries, the pain proves irritation of the sensational nerves; but what proof have we that such excitement of the sensational nerve momentarily palsies the vascular nerve? Henle ascribes it to a principle of antagonism between the two, which is merely another mode of stating the fact. Were there any such principle so easily called forth, nutrition would be subject to constant interference from sensation. Nor could it happen without our assuming, either that the same nerve tubes, which in the ganglion prevented action, in the spinal chord excited action, or else, that the sensational nerve tubes as they passed through a ganglion, themselves carried away the ganglionic energy, which, from the nature of the connection, is improbable, and which, as regards the lenticular ganglion, I believe, is proved not to occur. Dr Billing accounts for blushing by supposing that the emotion causes abstraction of nervous energy from the blood-vessels of the face, and thus permits their passive dilatation. But in terror we have a more intense emotion with a bloodless face. Here, according to the same hypothesis, there must be over-action of the vascular nerves of the face. Is it not more reasonable and more in accordance with their influence on other parts, to assume that depressing feelings diminish, and exciting emotions increase innervation for the time? Shame, the only moral case of blushing, is certainly not a depressing emotion; witness the action of the heart. Not, only, therefore, is this supposed suspension of vascular nervous energy not proved, but it is less probable than the "active dilatation" of J. Hunter. If a frog's foot be stretched and examined by the bright sunlight under the microscope, it will be noticed that there is no uniformity of rate between the flow of blood corpuscles in the capillaries and that in the arteries. After coursing on more slowly than before, then oscillating, the red globules in a given capillary tube will occasionally course back again towards the artery, and continue to do so with great rapidity for a minute or longer, then stopping, oscillating, and again resuming their course down the tube towards the vein. On the occurrence of any obstruction the globules, before becoming impacted together in the tubes contiguous, appear to be driven with force against the obstruction; they then recoil for some distance, and rush against the obstruction several times before they lose motion. During these varied directions and kinds of movement on the part of the blood globules, the capillaries appear to undergo no alteration in their dimensions. Consequently, the cause does not consist in any contraction or dilatation of the containing tubes. As the globules occasionally move against the direction of the *vis a tergo* just as freely as in the opposite direction, the action of the heart cannot be the influential agent. To ascertain whether we could affect these movements in the capillaries through the medium of the nerves, after examining the web of a frog's foot for some time, I exposed the lower part of the spinal chord and irritated the spinal ganglia on the side I was examining. The capillaries of the web were distended, obstructions from the aggregation of blood globules existed in several spots, and circulation was languid. At first I imagined the irritation of the ganglia might have caused something of this, but on exposing the web of another frog's foot in the same way to the light and heat of the sun for the same length of time, the state produced so nearly resembled that above described, that the observation as regarded ganglionic influence was evidently null. I have not succeeded better in other attempts to elicit ganglionic action on blood-vessels; but believing that we cannot in experiments induce the action of the gray cells of nervous centres, such a result is negative only, and of no weight until we have shown that some other effect can be produced in this way. This has not at present been done. Experimental physiology has so far acquainted us only with what the ganglia will not do; but that, if free from fallacy, is certainly an important step gained.

Are the attractions going on in the capillaries due to the vascular nerves? It is more in accordance with the facts of the vegetable and inanimate kingdoms to consider the affinities exercised in the capillary vessels as special attributes of the molecules which manifest them, and not as mere investments of these molecules by the nerves; just as muscular contraction and cell secretion are essentially the properties of their respective tissues. But just as these last are in a great measure controlled and affected by the nervous system, may be the capillary actions also. There are several modes conceivable in which this controlling power might operate. Energy of function depends on activity of molecular change in the organ engaged; this in turn depends on a due supply of pure blood, and in the few instances in which an organ has been seen in the active discharge of its function, it has been seen to receive at the time an unusual supply of blood.* Mere alteration of calibre in the capillary tubes, therefore, would affect the discharge of function in an organ; contraction of the capillaries lessening the amount of molecular change in proportion to the diminution in the quantity of blood received; dilatation of the capillaries increasing function in proportion to the increase of purified blood admitted. There is no instance in the economy of any part being endued with power expressly for the quiet and normal performance of its function only, and divested of that power whenever an augmented discharge of function was required. Yet this must be the case, if we admit that the capillaries are kept in a state of normal contraction by the force of the vascular nerves, and that their equally normal distension during functional activity is due to temporary removal of that force. Blushing, erection, neuralgic flush, counter-irritation, and the concomitant enlargement of vessels and local growth during gestation, during the budding of the deer's antlers, and in the generative organs during the procreative period in animals subject to periodical sexual excitement, are all better explained by supposing that excitement of the vascular nerves and increased afflux of blood go together, than the contrary. Could excitement of the vascular nerves cause the active dilatation of the walls of the capillaries? Notwithstanding the active elongation of the superior oblique muscle of the eyeball, and the active expansion of muscular canals as the result of spinal excitement, have been defended, they are not proved, not required, and against analogy. The iris and perhaps the heart are not exceptions, since, if these do enlarge actively, it is certainly not through any over-action of the same fibres which cause at other times their contraction in size. It is not proved that the capillaries are muscular tubes. I have never seen them manifestly contract on the application of a stimulus that could not act chemically on the surrounding tissue, and the contraction—always slight—which many have described as the immediate result of stimulation, may have commenced in the small arteries and subsequently appeared in the capillaries from diminished contents only. The contractility of the smallest bronchial tubes, and the non-contractility of the air-vesicles, is perhaps a type of a similar arrangement of the smaller arteries and the capillaries.* If this be so, dilatation and contraction of the capillary vessels must equally depend on the quantity and the pressure of the fluid they contain, which must, in turn, vary with the force of the heart's action, the tone of the large vessels, the quantity of blood in the system, and the amount of attraction going on between the blood and the walls—whatever their nature—of the capillaries themselves. Finding, therefore, that contraction of capillary vessels during functional activity is opposed to the fact of hyperæmia existing at that time; passive enlargement and active dilatation, alike opposed to analogy and insufficient for the explanation of all the phenomena produced, we infer that it is not by causing altered calibre of the capillaries in the first instance, that the vascular nerves indirectly influence the changes therein occurring.

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* Dr Combe; Andral; Beaumont.

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* Capillaries are mere tubular excavations in tissues without any distinct membranous walls, (Mr Addison;) they possess distinct membranous walls, presenting remains of nucleated cells here and there in their substance, (Henle;) not merely distinct walls, but a double muscular coat, consisting of transverse and longitudinal fibres, (Dr Hughes Bennett.)

For anything we know to the contrary, the nervous influence may cause contraction of muscular fibre by calling natural affinities into play. There is therefore nothing extravagant in assuming that the vascular nerves may cause an afflux of blood by exciting an increased exercise of the attractions and repulsions naturally and independently going on in the capillaries. Whether these molecular actions depend upon the conversion of blood into tissue, the separation of secretions in secreting organs, the change of arterial into venous blood, or, as is most probable, upon all these processes together, it is fully more philosophical to suppose that changes so extensive though so minute would be attended by some manifestation of attraction, than not. We have proof that these molecular changes, and the phenomena of attraction which attend them, can be affected in various ways. The elaboration of chyle and its conversion into blood take place doubtless without any assistance from the nerves. The constancy of such an arrangement as shall insure the passage of the chyle through numerous minute vessels before its reception into the blood, certainly implies that some kind of influence is exercised upon the chyle through the walls of the lacteals, especially in the mesenteric glands. There is no reason, however, for ascribing this influence to the nerves. With more probability it may be considered analogous to the changes occurring in the placenta between the impure fœtal and the purer maternal blood. The effect of animal poisons, and the alteration in the relative amount of the different constituents of the blood under various forms of diseases, the sudden augmentation of fibrin in sthenic inflammations, for example, evince a susceptibility to be readily affected in undergoing their changes, on the part of the blood-molecules. Similar facts occur in the vegetable kingdom. Humboldt noticed that, in a lettuce or euphorbium destroyed by a lightning, there is no such oozing out of milky fluid from the cut stalks as otherwise takes place. Here electricity instantly destroys vital molecular changes analogous to those presumed to occur in the capillaries of an animal. If the electric agent can do this, surely the equally subtle nervous force must have a similar power. The abolition of muscular contractility, the cessation of the circulation through the capillaries, and the non-coagulability of the blood simultaneously and instantly induced in some cases of sudden death from shock, which could act only through the medium of the nervous system, prove that it has. If, acting in an extreme degree, the nervous system can instantly and completely put a stop to all the molecular actions of life, when acting in a less degree it must be capable of modifying them. As an hypothesis, therefore, capable of explaining all phenomena referrible to the nervous system, so far as physical action is concerned, and apparently open to fewer objections than any other, we infer that the nervous force can stimulate every living molecule endowed with a certain function, to the more active discharge of that function; that it can consequently excite, variously modify, or greatly derange, all vital actions; that muscular contraction, cell-metamorphosis, the evolution of animal heat, are influenced by the nerves in precisely the same manner, the difference in result being due to inherent difference of vital endowment in the molecule subjected to the nervous influence; that the varied fulness and therefore the calibre of the capillary blood-vessels is secondary to, and dependent upon, the activity of the molecular changes going on in their contents, and under the control of the nerves only in so far as these affect the vital actions of the blood.

The proofs that the ganglionic nerves are the organic nerves; the relation of the ganglia to other nervous centres; and the extent of their normal influence on the vital processes, remain to be considered.

Holmes Chapel, Cheshire,
May 16, 1846.

ART. VII.—*Statistics of the Royal Infirmaries of Edinburgh and Glasgow; prepared from the Managers' printed Reports, and other authorized Documents.* By J. THOMSON, Esq.

THE object of the following tables is to show the result of the medical treatment, and the expense of maintaining patients in the Royal Infirmaries of Edinburgh and Glasgow; and as this is a second series of that inquiry, it has been thought advisable to follow the same plan as was adopted in 1842, (see Edin. Med. and Surg. Journal, No. 157,) as in this way a comparison between 1842 and 1845 will be easily practicable.

In the "Report of the Managers by the Royal Infirmary of Edinburgh for the year from 1st Oct. 1843 to 1st Oct. 1844," the contributors are informed that "certain alterations in the management and superintendence" of the hospital had been made, and "a number of bye-laws and regulations adopted," "which, without diminishing the comforts of the patients, will be found to operate beneficially as to the expenditure and superintendence of the establishment."

And as these alterations and regulations were in full operation during the year 1845, "while provisions were abundant and at reasonable prices," and everything done "to promote economy in the administration of the affairs of the Infirmary in every department," the following numerical statements have been prepared to show how far these changes have lessened the expense or bettered the situation of the patients.

TABLE A.—*Vital Statistics.*—Showing the number of patients treated in the Royal Infirmaries of Edinburgh and Glasgow, and in seven other hospitals; with the average residence in hospital; the number of deaths, and ratio of mortality per cent.; and proportion of deaths to cures, &c.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Hospitals.	Population of respective towns in which the hospitals are situated. Census 1841.	Period one year, ending	Under treatment during the year.	No. of cases treated to a termination.	Died.	Cured.	Relieved and discharged.	Disc. from other causes. viz. by desire, irreg. and improper. No benefit.	Average daily number in hospital.	No. of days in hospital for each cure.	Average residence in hospital in days.	Ratio of deaths per cent. of cases terminated.	Ratio of cures per cent. of cases terminated.	Proportion of deaths to cures.	Days of residence in hospital for 1 death.	Ratio of admission per cent. of the population.
Royal Infirmary Edinburgh.	164,451	Oct. 1, 1842.	3770	3529	443	2255	551	280	303	41	29	12.55	63.89	1. to 5.09	249	2.3
		Oct. 1, 1845.	3535	3278	369	2047	578	284	276	49	28	11.28	62.44	5.54	273	2.1
Royal Infirmary Glasgow.	173,147	Dec. 31, 1842.	3612	3405	328	2646	226	205	228	31	23	9.63	77.70	8.06	254	1.3
		Dec. 31, 1845.	3200	2993	309	2144	317	223	234	40	26	10.32	71.63	7.00	276	1.1
Aberd., Dumfries, Montrose, Dundee, Inver. and Perth.	185,629	1842.	4575	4230	223	3372	345	290	309	33	24	5.27	79.71	15.12	506	2.4

Observations on Table A.

Column 1.—Shows the number of the population in the different towns according to the census of 1841. The population assigned for Edinburgh includes Leith.

Column 2.—The period comprehended in this table is two years for Edinburgh and Glasgow, and only one year for the hospitals of Aberdeen, Dumfries, Montrose, Dundee, Inverness, and Perth. The former two being for the years 1842 and 1845, and the others for 1842 only, as specified in this column.

Column 3.—This column includes the cases remaining at the end of the former year, together with the numbers admitted during the annual period.

Column 4.—This column shows the number of cases under treatment during the year,—minus the numbers remaining in hospital at the end of the year.

Column 5.—Shows the number of deaths during the annual period.

Column 6.—The number of cures or recoveries.

Column 7.—Requires no explanation.

Column 8.—The purport of this column is obvious.

Column 9.—This column is of much importance in financial calculations. It shows the mean, or average daily number of patients in hospital,—which alone controls the expenditure.

Column 10.—This column shows the mean number of days of residence in hospital for each recovery. It is computed by dividing the days of residence by the number of cures, and the quotient or number produced, shows the relation of sick time to each recovery. The range in this column is from 33 days, the average of the *small* hospitals in 1842,—to 49 days in the Royal Infirmary of Edinburgh in 1845—while in Glasgow, both in 1842 and in 1845, cures or recoveries were effected—nine or ten days sooner than in Edinburgh. To what circumstance is this result to be attributed ?

Column 11.—Shows the average residence of patients in hospital. It is computed by dividing the days of residence in hospital by the number of patients under treatment during the year. In 1842, patients were six days longer in the Edinburgh Infirmary than in Glasgow; but in 1845, they were only two days longer than patients in the Glasgow Infirmary, and four days longer than the average residence in all the other hospitals during 1842.

Column 12.—Shows the ratio of mortality among the patients whose cases have terminated. The range of mortality being from 5.27, the average mortality of the six provincial hospitals, and 12.55 in the Royal Infirmary of Edinburgh in 1842. In 1845, the mortality in Edinburgh is 11.28 per cent. or about 1 per cent. more than in Glasgow, and 114 per cent. more than the average mortality of the small hospitals. This subject is one of much importance and calls for careful investigation.

Column 13.—Shows the ratio of cures or recoveries among the patients whose cases have terminated. In Edinbugh the cures are 9 per cent. *less* than in Glasgow, and 17 per cent. less than the average of the small hospitals.

Column 14.—Shows the proportion of deaths to cures. In the Edinburgh Infirmary for 1842, for one that died, 5.09 were cured, and in 1845 for every one that died, 5.54 were cured. In Glasgow, seven were cured for one that died in 1845. While 15.12 were cured in the *small* hospitals for one that died in 1842.

Column 15.—This column indicates the average number of days of sickness, or residence in hospital which occurs for one death. It is found by dividing the days of treatment by the number of deaths. In the Royal Infirmary of Edinburgh there is one death for every 273 days of sickness, or residence, and one death for 276 days of sickness in Glasgow,—while there is 506 days of sickness, for every death in the small hospitals.

Column 16.—Shows the ratio of admission to hospital per cent. of the population of the town in which the hospital is situated. Supposing the health and hospital accommodation nearly the same in Edinburgh, Glasgow, and other towns, this column will indicate the *intensity* of the diseases of the patients in these hospitals: and as the *worst* case is *first* admitted,—consequently, where only 1 per cent. of the population can be admitted to hospital, that case is necessarily the worst that offers. But in the hospital where 2 per. cent. of the community is admitted, the second case will be a shade less severe than the first, while a third case, and a fourth case, would be less and less severe still—and seeing that only 1 per cent. of the population of Glasgow, had been admitted to the infirmary in 1845, it is reasonable to suppose the cases treated in that hospital that year, had been a shade *more* severe than the cases which were treated in Edinburgh—where 2 per cent. of the population was received into hospital.

" These numerical results, founded upon data which cannot be disputed, deserve to be critically investigated, not only by gentlemen of the medical profession, but by philanthropists who wish to promote the happiness and the welfare of their fellow creatures.

TABLE B.—Statistics of Expenditure of the Royal Infirmary of

Number.	Classes of charge.	Edinburgh. For the year 1842.			Edinburgh. For the year 1845.			Glasgow. For the year 1842.			Glasgow. For the year 1845.			Comparative statement, showing the average yearly charge under each head, or class of expenditure, for 100 occupied beds in the hospital of Edinburgh. Fo the year 1842.			Edinburgh. 1845.			Glasgow. For the year. 1842.			Glasgow. 1845.		
	Number of patients under treatment during the year, ...	3770			3535			3612			3200														
	Daily average number of patients in hospital,	303			276			228			234														
A.	B.	C.			D.			E.			F.			G.			H.			I.			K.		
		L.	s.	d.	L.	s.	d.	L.	s.	d.	L.	s.	d.	L.	s.	d.	L.	s.	d.	L.	s.	d.	L.	s.	d.
1	Bread, biscuit, and flour, . .	926	19	2¾	663	15	2	375	8	3	448	2	6	305	18	6½	240	9	10	164	13	0	191	10	1
2	Oat, barley, and pease meal, . .	233	1	0	59	17	0	130	19	3	156	0	6	76	18	3½	21	13	8	57	8	9¼	66	13	5
3	Milk and small beer, . . .	848	17	6	358	17	0	277	9	10	367	17	11	280	3	1¾	130	0	4	121	14	1½	157	4	5
4	Butcher meat, poultry, hares, and rabbits,	944	10	9½	950	10	8	507	2	3	718	6	0	311	14	7	344	7	11	222	8	4¼	306	19	4
5	Tea, coffee, eggs, sugar, butter, cheese, ham, fish, and groceries, . .	461	0	6¾	652	3	10½	281	0	1	372	0	1	152	3	1½	236	6	0½	123	4	6	159	0	0
6	Pot barley, vegetables, potatoes, and other articles of food, ...	147	19	9	156	11	3	114	17	8	80	6	4	48	16	10	56	14	1	50	7	9	34	6	5¾
7	Medicines and drugs of all sorts, .	417	17	4½	591	15	11½	234	0	2	224	14	6	137	18	2½	214	8	4½	102	12	8¼	96	0	8½
8	Surgical instruments, bandages, and articles for the apothecaries' shop, . .	224	12	1½	159	1	11	161	1	0	204	13	11	74	2	6½	57	12	10½	70	12	8½	87	4	5
9	Salaries of medical officers and clinical lecturers, ...	198	2	0	152	2	0	659	10	6	701	11	0	65	7	7¼	55	0	8½	289	5	3½	299	16	3¾
10	Salaries of non-medical office-bearers,	456	6	9	612	8	8¼	415	0	0	267	15	0	156	12	1¼	221	17	11	182	0	4	114	8	5½
11	Wages of nurses, porters, and female servts,	892	6	8	829	15	6½	806	6	4	669	13	5	294	10	0	300	12	10	353	12	11¼	266	3	8
12	Wine, spirits, ale, porter, and vinegar,	482	18	2	287	15	10	165	5	2	350	11	0	159	7	6¼	104	5	5¼	72	9	7½	149	16	1¾
13	Advertising, ...	29	10	6	4	16	0	30	15	6	30	8	0	9	14	10	1	13	6	13	9	11¼	12	19	9¾
14	Printing and stationery, . . .	345	5	8	273	12	5	107	18	0	83	15	6	113	19	1¼	99	2	9	47	6	6	35	16	0¼
15	Heating and lighting, . . .	568	14	5½	761	13	1	363	10	10	320	7	6	187	13	11¼	275	19	2¼	159	8	11½	136	17	9¾
16	Repairing furniture and furnishings,	430	4	1	207	16	2½	398	13	10	204	1	8	141	19	7½	75	5	10¾	174	17	3¼	87	4	3½

TABLE B.—*Continued.*—Statistics of Expenditure of the Royal Infirmary of

Number.	Classes of charge.	Edinburgh.						Glasgow.						Comparative statement, showing the average yearly charge, under each head or class of expenditure, for 100 occupied beds, in the hospital of											
		For the year 1842.			1845.			For the year 1842.			1845.			Edinburgh.						Glasgow.					
														For the year 1842.			1845.			For the year 1842.			1845.		
	Number of patients under treatment during the year.	3770			3535			3612			3200														
	Daily average number of patients in hospital.	303			276			228			234														
A.	B.	C.			D.			E.			F.			G.			H.			I.			K.		
		L.	s.	d.	L.	s.	d.	L.	s.	d.	L.	s.	d.	L.	s.	d.	L.	s.	d.	L.	s.	d.	L.	s.	d.
17	Miscellaneous expenses. . . .	226	11	2	628	16	11½	375	18	8	301	5	2	74	15	5½	227	16	10¼	164	17	6¾	128	14	10½
18	Total ordinary expenditure. .	7834	17	9½	7551	9	6¾	5407	17	3	5501	10	0	2585	15	5½	2663	8	9½	2370	10	3¾	2350	16	3
19	Feu duties, annuities, and interest.	543	2	10	117	19	6½		...			...		179	5	1	42	14	10¾		...			...	
20	Building, alterations, and repairs. .	887	10	6	340	19	1	534	19	0	262	18	0	292	18	3	123	9	2¾	234	19	6¾	112	7	0
21	Grand total expenditure. . .	9265	11	1½	7810	8	2¼	5942	16	3	5764	8	0	3057	18	9½	2829	12	4	2605	9	10½	2463	3	3
	Yearly average expense of each occupied bed for provisions. Nos. 1, 2, 3, 4, 5, 6. .	11	15	1	10	15	11	7	7	11¾	9	3	1	1175	14	6¼	1029	11	10½	739	16	6	915	13	8¾
	Yearly average expense of each occupied bed for medicines, instruments, salaries, wages, wine and spirits. Nos. 7, 8, 9, 10, 11, 12.	8	16	6	9	10	9½	10	14	1¾	10	6	8¼	882	10	0	953	19	2	1070	14	7	1032	8	9
	Yearly average expense of each occupied bed for the total ordinary expenditure. No. 18.	25	17	1¾	26	12	8½	23	14	4½	23	10	2½	2585	15	5½	2663	8	2½	2370	10	3¾	2350	0	10
	Average yearly expense of each occupied bed for the grand total expenditure, which includes building and repairs of property. No. 21.	30	11	7	28	6	0	26	1	3½	24	12	8	3057	18	9½	2829	12	4	2605	9	10½	2463	3	3
	Average cost of the food of every patient who is admitted to hospital. Nos. 1, 2, 3, 4, 5, 6.	0	18	10¾	0	16	1	0	9	4	0	13	4½	94	9	7	80	8	4	46	13	9	66	17	6
	Average actual expense of every patient who is admitted to hospital. No. 21. . .	2	9	1¾	2	4	2¼	1	12	10¾	1	16	0	245	14	7	221	2	11	164	9	7	180	0	0
	Daily average expense of each occupied bed for medicines and attendance. No. 18. .	0	1	5	0	1	5½	0	1	3½	0	1	3¼	7	1	8	7	5	10	6	9	2	6	7	1

Observations on Table B.

The foregoing table shows that 3,770 patients were admitted into the Royal Infirmary of Edinburgh, and 3,612 into that of Glasgow during the year 1842; and 3,535 into the Edinburgh Infirmary, and 3,200 into the Glasgow Infirmary in 1845,—while the daily average number of patients in the Edinburgh Infirmary was 303, and in Glasgow 228, in 1842; and 276 in Edinburgh, and 234 in Glasgow, during the year 1845.

Column A.—This column indicates the number of the classes of articles which form the expenditure in both hospitals.

Column B.—shows the name or description of the article.

Column C.—shows the amount of the charge for each class of articles, in the accounts of the Royal Infirmary of Edinburgh for the year 1842.

Column D.—shows the amount of the expenditure in Edinburgh for the same articles in 1845.

Column E.—This column indicates the amount of the charge for the same sort of articles in the Royal Infirmary of Glasgow in 1842.

Column F.—shows the amount of the expenditure, under the same heads, in Glasgow, for the year 1845.

Columns G. *and* H.—Both apply to the Edinburgh Infirmary. *Col.* G. is calculated to show the *average* yearly cost (under each class of charge) for 100 patients, in 1842, and *Col.* H. for 100 patients in 1845.

Columns I. *and* K.—are both applicable to the Glasgow Infirmary. *Col.* I. has been calculated to show the *average* yearly charge (of each class) for 100 patients in hospital in 1842, and *Col.* K. for the same articles and number of patients in hospital in 1845.

Columns G. H. I. *and* K.—indicate the yearly charge, under each head or class of expenditure, for 100 occupied beds in the same hospital, at different periods, and in other hospitals for the same period; which being seen at a glance, is of much importance as a check upon the expenditure. Seeing the same number of patients, under ordinary circumstances, ought to consume about the same quantity of provisions and other stores, and require as many nurses at one time, and in one hospital, as another, consequently the amount of the expenditure in one year and another, for 100 or any other given number of patients in a well-regulated hospital, should never vary very much beyond what could be accounted for by the state of the markets.

With these illustrations, one example, it is hoped, will render Table B. intelligible to any one who may take an interest in the subject. It reads thus:—In Col. C., opposite No. 1 in Col. A, the "bread, biscuit, and flour," for 303 occupied beds in the Royal Infirmary of Edinburgh in 1842 appears to have cost L.926, 19s. 2¾d., or L.305, 18s. 6½d. for every 100 patients in hospital, as shown in Col. G.; whilst the expenditure of the Glasgow Infirmary for the same articles, during the same period, as shown in Col. E., was L.375, 8s. 3d. for 228 occupied beds, or L.164, 13s. for every 100 patients in hospital, as shown in Col. I. Col. D. shows that the "bread, biscuit, and flour" for 276 occupied beds in the Edinburgh Infirmary during 1845 cost L.633, 15s. 2d., or L.240, 9s. 10d. for 100 occupied beds, as seen in Col. H.; while the cost of the same articles, during the same period, for 234 occupied beds in the Glasgow Infirmary, was only L.448, 2s. 6d., as stated in Col. F., or L.191, 10s. 1d. for 100 occupied beds, as shown in Col. K.

The sums charged in the accounts of the Royal Infirmary of Edinburgh for the year 1842, taken from Col. C., for "bread, biscuit, and flour; oat, barley, and pease meal; milk and small beer; wine, spirits, porter, ale, and vinegar," amount to L.2,491, 16s. 7¾d. for 303 occupied beds, or L.822, 7s. 5½d. for 100 beds; whereas the charges in Col. D. for the corresponding articles in 1845 is only L.1370, 5s. for 276 occupied beds, or L.496, 9s. 4d. for every 100 beds, being L.325, 18s. 1½d. *less* than the cost of 100 beds in 1842. And notwithstanding this large reduction in the cost of the provisions and nourishment of the patients, (*equal to nearly a half of what they cost in* 1842), yet the actual cost or yearly average expense of 100 occupied beds (exclusive of buildings, alterations, and repairs, feu-duties, annuities, and interest,) amounted to L.77, 13s. 9d. *more in* 1845 than in 1842. How is this accounted for? The subject is important; for, with these accounts before us, it is not easy to conceive how any fixed rule can be in operation, and rightly acted on, in delivering out the provisions and stores of the Royal Infirmary, or how any satisfactory explanation could be given of the disproportionate charges which appear in the accounts of different years, for the same article, to the same number of patients; and looking at the cost of an article, or class of expenditure, for 100 occupied beds, in the Glasgow Infirmary, and making allowance for the altered state of the markets, the disbursements of that hospital in 1845 bear a strong resemblance to their predecessors in 1842,—such as we are unable to trace in those of Edinburgh.

Altogether, one cannot but see that something is wanting in Edinburgh to regulate and control the deliveries of provisions, stores, and other articles, according to the daily number and the state of health of the patients; and there is nothing so likely to effect that object as the adoption of a proper and strictly enforced system of diet-table rolls, similar to those of army hospitals, whereby waste of every kind would be prevented, and the actual expense of each individual shown,—as a boarder, a servant, or a patient, as the case might be.

The expediency of proper checks to enable the functionaries to regulate and control the expenditure of the Royal Infirmary of Edinburgh is obvious, seeing that the "milk and small beer" for 100 patients cost L.280, 3s. 1¾d. in 1842, and only L.130 in 1845: that the expense of "heating and lighting" that hospital in 1842 was L.568, 14s. 5½d., and L.761, 13s. 1d. in 1845, while the charge for "medicines" and other articles fluctuate nearly as much between one year and another. Nor is a comparison between that hospital and other hospitals more satisfactory. For example, the "medicines" for 100 occupied beds in the Royal Infirmary of Edinburgh in 1845 cost L.214, 8s. 4½d., while the "medicines and drugs" for the same period and number of patients in the Glasgow Infirmary cost L.96, 0s. 8½d., showing that L.118, 7s. 8d. *more* is spent on the medicines of every 100 patients in Edinburgh than in Glasgow. How is this accounted for? The ratio of the recoveries is greater, and the mortality less, in Glasgow than Edinburgh.

Proper diet-tables and other returns would lead to a better system, and enable the officials to control the expenditure, from the facilities they would have for comparing one article and one year's expense with another, and neither mislead themselves nor lead the managers to "believe they have been successful in promoting economy in the administration of the affairs of the Infirmary in every department," as on page 4th of the Report for 1845,—a year when the total expenditure applicable to 100 occupied beds exceeded that of the preceding year by L.618, 9s. 7¼d.; the cost of 100 patients in 1845 being L.2829, 17s. 2d., and in 1844 only L.2211, 7s. 6¾d., notwithstanding the "discharge applicable to 1844 was *increased* by several large accounts, which properly belonged to former years,"—a thing which cannot apply to the expenditure of 1845.

PART II.

CRITICAL ANALYSIS.

Art. I.—*Scrofula; its Nature, its Causes, its Prevalence, and the principles of Treatment.* By Benjamin Phillips, F. R. S., Assistant Surgeon to the Westminster Hospital. Illustrated with an engraved plate. London, 1846. 8vo, Pp. 379.

Struma and diseases ascribed to the strumous disposition prevail so extensively, and are so unmanageable, that it is not wonderful to observe that they from time to time give rise to the appearance of various new treatises on the nature, the causes, and the treatment of these disorders. It is indeed true that certain periods seem to be more prolific in the production of these works than others; and though the causes of this occasional profusion of writings on one subject may not at all times be very manifest, yet it is generally possible to trace them to circumstances, the operation of which is sufficiently manifest. Thus the introduction of new or powerful remedies and new methods of treatment generally produces a considerable number of essays, memoirs, and treatises; and hence a considerable proportion of essays and treatises are more or less connected with the appearance of remedies of considerable reputation. The alkalis gave rise to the appearance of various essays; muriate of lime and muriate of baryta produced a few; iodine and its various forms of administration formed a prolific source of essays on struma and strumous diseases; and cod-liver oil and bromine have not been without their results.

In other instances we find, that in the lapse of years, in the rise and fall of medical theories and opinions, the views of medical practitioners become more rational and more philosophical than previously. Taking more enlarged and correct views of the origin of the disorder, they look less to specifics and peculiar modes of treatment, and more to the means of discovering the causes of the disease, and opposing and counteracting their operation, in suitable dietetic measures, and attention to the moral and physical training of the young of the human race. In the works and essays that have of late years appeared on this subject, this spirit is much more conspicuous than formerly. In various treatises that have recently come under attention, the authors have shown a desire to understand and discover what the elder pathologists called the *genesis,* or mode of formation of the disease, to distinguish the circumstances under which it arose, acquired strength and virulence, and affected the different textures of the human body; and not less carefully the circumstances under which its symptoms gradually subsided, and how it seemed to be finally overcome.

Connected with these inquiries, medical investigators have directed their attention to the question of the prevalence of strumous maladies among masses of individuals placed in similar circumstances; they have studied to discover the influence of the various circumstances of food, air, climate, confinement or freedom, moral causes, professions, and localities in the production of strumous disorders; in short, to ascertain by statistical inquiries what are the causes which favour, and those which oppose, the formation of the disease. We lately remarked that all investigation into the nature and origin of diseases has already reached that point that this mode of inquiry is either the only one or the most likely one to throw light on these questions. Struma and strumous disorders are exactly in this position; and from such inquiries alone can we hope to derive much advantage.

The treatise of Mr Phillips is very much of the kind now mentioned. Though devoted to the elucidation of the nature, causes, and prevalence of scrofula in general, and the consideration of the most effectual methods of treatment, yet the great part of the volume is occupied in the presentation of facts illustrating the causes and prevalence of the distemper.

Scrofula Mr Phillips regards as a disease of the constitution, indicating its presence and operation, nevertheless, by certain external signs, of which swelling of the subcutaneous lymphatic glands is the most conclusive. All forms of tumid glands, however, he does not regard as proof of a strumous constitution. The glands are known to become enlarged from irritation at their organic extremities, as the inguinal glands swell from a sore on the foot, the axillary glands from whitloe or a sore on the hand or fingers, and the mesenteric glands from ulcers of the intestinal mucous membrane. Unless the swelling of the gland is attended with the deposition of that sort of product which is known as strumous or scrofulous matter, the proof of the presence of the scrofulous constitution is, in the opinion of Mr Phillips, wanting.

Mr Phillips, further, does not allow these diseases to be scrofulous in which there is no scrofulous matter present; and all the disorders usually imputed to the strumous diathesis, as strumous ophthalmia, diseased joints, and similar affections, he regards as the result of such low inflammatory action as is often seen in a debilitated state of the constitution.

The author also calls in question the accuracy of many of the usual markings or external phenomena and signs, by which it is conceived the presence of the scrofulous constitution is denoted. He regards the indications drawn from the colour of the face and complexion as uncertain and fallacious; those of the *alae nasi* and upper lip as not invariable.

His own enumeration of the external characters is as follows:

"In the form of the body there is usually observable a want of muscular development, but even this is often absent. There is often an appearance of plumpness or roundness, which is the result not of muscular development, but simply of an hypertrophied, or infiltrated condition of the cellular tissue, and which rapidly disappears under fatiguing exercise, privation, or disease. Commonly there is a general paleness and coldness of the surface of the body, which is owing to a feeble circulating apparatus; but, in a large number of cases, about one-fifth of the whole, that paleness does not extend to the face. The colour of the hair is very variable, but for the most part it inclines to a dark tint. Of nearly 9000 scrofulous children, I have myself examined, a little over 32 per cent. had light hair and eyes. The *alæ nasi* may be broad, but for the most part they are not so; the upper lip or even both may be tumid, but in a majority of cases they are not so. There is not, as some persons have supposed, any thing constant in the shape of the lower jaw, or in the appearance of the teeth. The abdomen is commonly tumid. The whole of the mucous surfaces are especially liable to derangement; discharges from the nose, the eye, and the ear, are common. The digestive mucous membrane affords early indications of suffering; the tongue has commonly a dirty whitish coating, the tonsils are usually enlarged, and they are often so tumid as to impress a disagreeable and frequently husky character upon the voice, and to cause snoring when the patient is asleep. A still more deleterious influence is exercised by these tumid bodies; they lessen so much the channel for the passage of the air in respiration that the sufficient development of the chest may be interfered with. The stomach and bowels are frequently disordered, and digestion is ill performed; acrid eructations are common, flatulence is often very troublesome, and the action of the bowels is very irregular, sometimes relaxed, at others constipated; sometimes the evacuations are clay-coloured. very offensive, and of varying consistency; at others, having a redundancy of bile. Similar evidences of derangement are observed in the air passages, commencing at the nose, (which exhibits increased secretions upon the occurrence of very slight variations in temperature,) and passing through their whole length. Similar phenomena are observed in the mucous tissue of the genito-urinary system; the bladder often shows an impatience of the presence of the urine, and the desire to void it is often frequent. The skin, though often dry and hard, is sometimes the seat of a considerable greasy exhalation; sometimes it is found to be fetid and sour. The acidity of the exhalation may be so decided as to determine a reaction upon litmus paper; in many of the cases observed by Mr Kaye on the Mediterranean coast it was so. The scalp and other parts of the cutaneous integuments are often the seat of eruptive affections. The absence of vascular and muscular energy often causes the child to lie and sit about much, and indisposes him to enter into the energetic games of his playfellows. As to the intellectual development claimed for scrofulous persons, I am bound to say that it is usually wanting. That many scrofulous children present that character is quite true; but the result of very careful observation has convinced me, that the overwhelming majority are without those superior intellectual qualities which have been pointed out as their ordinary character. Among the better classes, the feebleness of a scrofulous child attaches to him an interest which, without it, he might not have enjoyed. To compensate for his physical inferiority, the anxious parent seeks to make him mentally superior to his bodily stronger fellows, and frequently succeeds; but often the limit of healthy action is passed, the nervous and intellectual systems have the vital action concentrated on them too intensely; the sufferer loses flesh, the general health languishes, and the intellectual faculties may give way, destroyed by an opposite, but not less sure method than that which breaks down the poor man's child.

"Where we find persons tainted with scrofula living under less favoured circumstances, the picture to be drawn of their physical and intellectual characters is widely different. In the cottages of the poor we find the child with the scrofulous diathesis often pallid, puffy, insensible listless, and filthy; the skin dry, harsh, and too commonly covered with eruptions—the mucous surfaces deranged, the attention not easily fixed nor even excited—the senses obtuse, the mind greatly wanting in intelligence, unimpressionable and almost incapable of action. The fact is, that those children who are surrounded with comforts and discreetly treated, and whose condition makes them objects of interest to those around them, present very different features of the disease from those which mark the scrofulous child of the poor. By the poor such a child is regarded as a calamity; while the other children are at work or play, he languishes in the corner of a solitary home; and if he be not altogether deprived of force and energy, what remains is soon wasted by taxing him beyond his powers.

In a constitution favourable for the deposit of scrofulous matter, I believe there are no features, in the absence of the tumour, so constant and so conclusive as to justify a reliance upon them, in pronouncing an opinion whether a constitution be scrofulous or not. It is certain that the ordinary tests are fallacious; I know that the major part of them may be observed, again and again, without any other evidence that the constitution is tainted with scrofula. We may even have enlarged glands, while no product such as that which I have alluded to, is deposited; although, in the absence of any source of irritation, enlarged subcutaneous glands constitute grounds for grave suspicion that the constitution is scrofulous. Thus, whatever may be the constitutional peculiarity, however marked may be the general physiognomy by what is called the scrofulous diathesis, we have no certain sign of the existence of the disease until sufficient evidence is obtained that the deposit has taken place. The constitution may suffer long before such a deposit it made, and the glands themselves may be swelled without presenting in their substance a scrofulous deposit; indeed the deterioration of the system proceeds so slowly, that although the tendency be directly onwards from the period when the gland is simply enlarged, to that when the deposit would ordinarily occur—in that interval favourable or unfavourable circumstances may be experienced, and no deposit may take place;—on the one hand, the constitution may improve and the glandular swelling may subside; on the other, the ailing child's life may be cut short by other diseases before the proof of scrofula is complete."—Pp. 30—33.

The existence of the diathesis or disposition, however, he does not altogether deny; but maintains that its existence can be known only by open effects. Connected with this, also, he allows a deterioration in the solids and fluids, a considerable deviation from health in the structure and functions of parts. The muscular system is not energetic, and certainly in its actions not capable of much endurance; and indications of the lymphatic temperament may be recognised in the facility and rapidity with which strumous subjects become exhausted under labour, fatigue, and privations.

All this change the author seeks to trace to some of the forms of impaired digestion; and it cannot be denied that most of the persons presenting the strumous habit or actual strumous disease, exhibit, at the same time or previously, various indications of disordered, irregular, or impaired digestion. The sour exhalation of the breath, the acid state of the skin, and the readily acescent condition of the urine, are well known in such subjects.

As to the strumous deposit itself, he represents it in the glands in which it is first formed, as attended with a change of structure in the glands, which he says depends on increased action alone. At an early period, in certain cases, the matter deposited presents the appearance of gray translucent matter, like tubercular matter in the lungs. But the most frequent course is for the scrofulous matter to be deposited as opaque, amorphous, gray, or yellowish masses, irregularly granular, and not unlike moist old cheese, in the enlarged glandular tissue. According to the best accounts, when this substance is subjected to microscopical inspection, it consists of granular molecules, irregular exudation corpuscles, rarely with nucleolus, but containing oil globules:—a minute granular matter, oily spherules, shapeless albuminous flakes or shreds, and a few irregular corpuscles;—but no regular or uniform structure.

It appears that, chemically viewed, these substances are albuminous, with an excess of alkaline salts, and that the albumen is crude or imperfectly developed, and sometimes that there are carbonates and phosphates of lime; and one authority, Bredow, regards the strumous matter as albuminate of potass or soda, in short, a combination of albuminic acid, and potassa or soda. Previous to the deposition of this new product, the gland or glands have been enlarged, sometimes to a great degree; and there is much increased vascularity, giving rise to its usual external effects. This enlargement is said to depend on the general parenchyma of the gland being in a state of chronic inflammation. This parenchyma is found on microscopic examination to be infiltrated with exudation corpuscles, resembling lymph globules.

The morbid product thus deposited or secreted is prepared, Mr Phillips argues, by the blood; and although he does not, like Lugol, infer that it exists in that fluid before it is deposited in the glands, yet he admits that the blood is much changed, and probably diseased in every instance of strumous deposition. One of the most common and uniform changes is an imperfect coagulability or a low degree of coagulating power, with more or less change in the shape of the lenticular globules, some of which are flattened and perforated like wheels, others irregular or notched. The author allows, nevertheless, that this is observed in anaemia generally, and that it is impossible to specify any condition certainly and positively characteristic of strumous blood.

He allows further, that it is impossible, in the present state of knowledge, to say whether the process of deposition be one of secretion or excretion; whether it occurs in consequence of some peculiar action developed in the part, and causing a new combination between the constituents of healthy blood; or whether it is owing to the blood having undergone the change before it has ceased to circulate, becoming overcharged with albumen and salts, and ready, when the ordinary excreting organs do not carry them off, to deposit them anywhere, wholly independent of local action. He concludes, however, "that the blood is changed before the deposit is made, that the accumulation of certain morbid materials in the blood constitutes what is known as the scrofulous diathesis or constitution; and that the deposition in the subcutaneous lymphatic glands constitutes what is known as scrofula."

As the definition given by Mr Phillips excludes phthisis from any necessary connection with scrofulous disease, he next considers the question, whether pulmonary tubercle and scrofula be in their nature identical? It is well known that the affirmative of this proposition has been maintained, or believed more or less confidently, by the great majority of pathologists and physicians; and perhaps, in proportion as experience has thrown light on the nature and mode of formation of the two diseases, and on the kind of subjects in which they take place, this proposition has acquired increased stability and certainty. It is well known that White, Lloyd, Hufeland, Laennec, Sir James Clark, Lugol, and many others maintain the identity in origin, and the close connection of the two diseases, pulmonary tubercle and scrofula; and almost all practical physicians are daily receiving more evident and more decisive proofs of this connection. Yet there are not wanting patrons of the doctrine that the two affections are not necessarily connected; and among them is the present author.

This difference, he thinks, may be maintained on the following grounds. First, tubercular matter is not the same as scrofulous matter; they are shown to be different by sensible examination, by microscopic examination, and by chemical constitution. The differences deduced from microscopical examination are doubtful; those from chemical characters not cognizable. Secondly, it does not appear possible to establish any decided difference between tubercular and scrofulous matter from the condition of the part in which the deposit is made, or from the vascularity or non-vascularity of the two deposits. The author then directs attention to a third point, viz. the difference as to the period when scrofula and phthisis prove fatal, as a source of distinction. The results of this comparison we must give in his own words.

"Mortality tables prove that the generally received opinion is correct, that the ravages of scrofula, where it destroys life, are most severely felt before, those of consumption after, the period of puberty.

"In the British Metropolis, the reports for 1840–1 show, that the deaths from consumption were 21,667; of those, 4653, or 21 per cent. died before fifteen. The deaths from scrofula, during the same period, were 347; of those, 208, or 60 per cent. died before fifteen. In the twenty-four districts of England and Wales, selected by the Registrar-General, the deaths from consumption in the same time were 8550; of those, 2543, or 29 per cent. were under fifteen. The deaths from scrofula were 106; of those, 61, or 63 per cent. were under fifteen. In Birmingham, the total deaths from consumption are stated at 668, of which 278, or 41 per cent. were under fifteen. The deaths from scrofula amounted to 3; of which 2, or 66 per cent. were under fifteen. At Liverpool, the total deaths from consumption were 1396; of which 540, or 38 per cent. were under fifteen. The cases of scrofula amounted to 10; of which 8, or 80 per cent. were under fifteen. At Manchester, the deaths from consumption are returned 1124; of which 421, or 37 per cent. were under fifteen. The deaths from scrofula were 10; of which 6, or 60 per cent. were under fifteen. The following table will show these results at a glance.

DEATHS.

CONSUMPTION.

	Total.					
Metropolis . .	21,667	of which	4653, or 21	per cent.	were under	15.
24 Districts . .	8,550	,,	2543, or 29	,,	,,	
Birmingham Liverpool Manchester .	3,188	,,	1239, or 39	,,	,,	

SCROFULA.

	Total.					
Metropolis . . .	347	of which	208, or 60	per cent.	were under	15.
24 Districts . .	106	,,	61, or 63	,,	,,	
Birmingham Liverpool Manchester .	23	,,	16, or 69	,,	,,	

" In Ireland, during the ten years ending in 1841, the tota deaths from consumption are stated to be 135,590 ; of which 10,779, or under 7·9 per cent. occurred before they attained the age of fifteen. The deaths from scrofula in the same period are stated to have been 3149 ; of which 1363, or 43 per cent. occurred before fifteen. It will be seen that in both cases the proportion is under that of England and Wales. It must, however, be borne in mind, that in the Irish Returns a very large number of deaths under fifteen are described as marasmus—68,650 ; being more than half the total deaths from consumption. Some of those cases were, no doubt, consumption, some scrofula, and some *tabes mesenterica.* But as we cannot state the real proportions, it will be better not to assume them.

" The researches made by order of the Count Chabrol show that in Paris, the greatest number of deaths from consumption happen between 20 and 30 ; then comes the period between 30 and 40 ; then 40 and 50 ; then 50 and 60 ; then 0 to 10.

" The proof, then, seems to me ample, that the period of life when the deaths from phthisis are most numerous is not that when the ravages of scrofula are most keenly felt ; and this constitutes another distinction between these diseases. I know that it is said that the registered deaths from scrofula are no proof of the extent to which it prevails, but such reasoning would also apply to phthisis. Complete evidence they may not furnish ; but as approximative evidence, they are unexceptionable. If the deaths from scrofula in a given district be 100, and in another district of similar population, 50, I conceive it would be abundant proof of the greater prevalence of the disease in the former district than in the latter ; though it might not show how far enlarged glands could be detected in either case."—Pp. 70–71.

We confess that we cannot agree with the author in the conclusion here deduced. Admitting the fact to be as the bills of mortality show, it appears to us that the difference as to the period of life only shows, that at one time one form of disease, at another another form of disease, may be more prevalent and more fatal. It has been a long admitted fact, that tubercular destruction of the lungs is before puberty a rare disease, and that after puberty it is a very common affection, perhaps the most common of all the disorders of the chest. It is also an admitted fact that strumous diseases of the glands, though liable to take place at all times of life, are greatly most common in infancy, childhood, and early life generally. Such is the mere fact. But it appears to us that it is impossible to deduce from these facts such conclusions as that the two disorders are different, merely because they prevail at different periods of life.

The relative prevalence of the diseases among the two sexes furnishes the next argument. He takes the returns as given in the reports of the registrar-general, and finds the deaths from phthisis among the male and female population for four years to be as follows:—

	1839.	1840.	1841.	1842.
Male,	28,106	24,519	24.329	24,408
Female,	31,453	28,168	27,937	28,098
Both,	59,559	52,687	52,266	52,506

This shows a preponderance of female mortality from phthisis of 15 per cent.

For the same years the mortality from scrofula among males and females respectively is as follows:—

Males,	679	658	616	678
Females,	472	547	472	508
	1151	1205	1088	1186

This shows, on the other hand, a preponderance of mortality among males by about 24 per cent. The Irish report gives the same result, the excess of female deaths above males in phthisis being 13 per cent., while the excess of male deaths from scrofula above female deaths amounts to 35 per cent. The author, therefore, concludes that while consumption is most fatal to the female population, scrofula is most destructive to the male.

On this point we have merely to observe that, in order that the conclusion be well founded, in all respects, it would be requisite to specify the ages at which these relative different amounts of mortality take place.

Mr Phillips next directs attention to the difference in the relative frequency of the two diseases.

From the registrar-general's tables it appears that the mortality from consumption during four years amounts to an annual average of 59,500, and those from scrofula 1200; the proportion which the one bears to the other being as 50 to 1; and the proportion they severally bear to the population as 1 to 265, and 1 to 13,255. Mr Phillips argues, that, if the causes of the two diseases be the same, this proportion ought to hold good in each district. In the north west district the deaths from consumption are 9976, those from scrofula 116; the proportion which they bear to each other is as 1 to 86, and to the population as 1 to 206, and 1 to 17,782. Here then the author infers, that while causes of consumption are in action with more, those of scrofula present 20 per cent. less intensity than the average of England and Wales. In the town population, selected by Mr Farre, embracing four years ending in 1842, there is a population of 3,759,186. The deaths from consumption are to the population as 1 to 235, while those from scrofula are as 1 to 20,000. In the country population, among a body of 3,446,501, the deaths from consumption amount to 1 in 286, those from scrofula to 1 in 10,000 or 100 per million. The deaths from consumption are, therefore, 19 per cent. greater in the town than in country districts, while those from scrofula are 100 per cent. less.

From all these facts Mr Phillips infers that, if the causes of the two affections be the same, the effects differ widely; where the mortality from consumption is great, that from scrofula is small, and the converse.

Mr Phillips also maintains that in India consumption is much less frequent than in Great Britain, while scrofulous disease is very frequent. In Russia, pulmonary diseases, and especially consumption, are much less frequent than in Great Britain; whereas scrofula is more prevalent and produces more dreadful effects.

"I know," says Mr Phillips, "it is said that scrofula does not commonly destroy life; that some other disease cuts down the victim of the disease; and that, therefore, the register does not show the amount of scrofula prevailing in any district. Whatever force there may be in that observation, it should follow, that if, as is alleged, the sufferer from scrofula usually dies of phthisis, the ordinary marks of scrofula should be apparent in the bodies of a large number of those who die of phthisis.

"Now for some time I kept a register of the cases of phthisis examined at the St Marylebone Infirmary, and the number included in that register is 332; of these seven only presented scars resulting from scrofula. In seven instances there was tumefaction of the cervical glands, but in only two instances did we discover that they contained scrofulous matter. To my own experience I may add that of Louis, who has published the result of the careful examination of 350 bodies of persons who died phthisical. He discovered a tuberculous condition of the cervical glands thirty-five times. That of Lombard, who examined 100 phthisical cases, and found the cervical glands affected seven times; and in seventy-three cases of children, seven times. That of Papavoine, who examined fifty tuberculous children, and found the same glands affected twenty-six times; and that of M. Cless, of Stuttgard, who lately gave the results of the careful examination of 173 tuberculous patients, from which it appears that he only detected tuberculous matter in the cervical glands six times. Here, then, we have the examination of 1078 phthisical patients, of whom it is said, 84 or 8 per cent., presented tuberculous disease of the cervical glands. There is so much discrepancy between these results, that it must very much detract from their value, unless there be some satisfactory mode of explaining that discrepancy. Lombard examined seventy-three children, and found 7 enlarged cervical glands, or 10 per cent. Louis examined 350 phthisical bodies, and found a-tenth with tuberculous cervical glands; now in the same country, and actually in the same city, Papavoine found 52 per cent. with tuberculous cervical glands, whilst out of 332 cases in which the patients died of phthisis, I have only found three instances in which scrofulous matter was contained in the cervical glands. As far as my own cases are concerned, I know that the results are accurately stated, and I am inclined to think that Papavoine—and the remark may possibly apply to others—has regarded a sensible enlargement of those glands as scrofulous, and has assumed that the characteristic deposit was present without careful examination.

" From these observations it results, that in the cases of those who die of tubercular phthisis, numerous as they are, the proportion which those who exhibit any marks of having suffered from scrofula bear to the total deaths from phthisis is very small."—Pp. 74.

Mr Phillips next appeals to comparative pathology, and the evidence afforded by the morbid state of the lower animals. From this it appears to be a rare malady among dogs. Sheep, it is well known, become affected with tubercular disease if kept in damp or irrigated pastures; and the rabbit in like manner, if confined and fed on watery food, becomes attacked with tubercular deposits. But this, Mr Phillips maintains, is not scrofula; and scrofula among brute animals is rare. Even the hog, from which the disease has received its denomination, does not present the marks of scrofula properly so called; and the measly affection, as it is named, to which these animals are most subject, consists in the generation of the *echinococcus* in the cellular tissue and skin. This, however, has no resemblance to struma; and in these animals the strumous affection of the glands is not observed.

Lastly, Mr Phillips states, and he quotes in support of the accuracy of his statement, the authority of Professor Sewell and Mr Simonds, the last of whom says that "he has never seen scrofula in the ruminants."

To us it appears that, so far as we understand the reasoning here pursued, these conclusions must be received with very considerable caution. We have seen how Mr Phillips defines and limits the character of scrofula; but even this definition and limitation would not, according to our observation, lead to the inference that the lower animals in general and the ruminants in particular, are quite exempt from enlargement of the glands. There is no doubt that, generally speaking, the lower animals do not very frequently present diseases of this class; and probably, while they are living in a state of nature, they are very rarely, perhaps never attacked by the disease. But that enlargement of the glands with deposition of tubercular or albuminous matter does never take place in the ruminants, is what we think cannot be admitted. Dupuy has recorded very distinctly several instances of this change in the lymphatic glands in various species and genera of lower animals. But even if this evidence we had not, we can say that we have seen enlargement of the bronchial lymphatic glands in the ruminants, and enlargement which we think must be regarded as scrofulous. Indeed we cannot see what else it can be called. This disease takes place in the ox; and a well marked instance of it among others which have been seen and noticed, is recorded in the sixty-third volume of this Journal, p. 98.* In that case, no doubt could be entertained either of the fact that the bronchial glands were enlarged with a tyromatous, or caseous, or scrofulous deposit, and that this change could not be ascribed to any other morbid influence than that of scrofula. Until, in short, facts of this kind are explained in some other mode, we cannot agree with the inference of the author, that scrofula or scrofulous disease does not occur in the ruminants.

Mr Phillips considers next the prevalence of scrofula in Great Britain.

From the examination of children in schools, manufactories, and similar institutions in England and Wales, he finds that among the children of the poor, the proportion between the ages of five and sixteen who present marks of scrofula, evident upon simple inspection, amounts as nearly as possible to rather less than 3½ per cent. The marks recognized by Mr Phillips in this estimate are, glands sufficiently enlarged to be obvious at sight; scars resulting from the suppuration of such glands; or diseased bones and joints, evidently scrofulous in their character. This proportion, however, does not represent the actual prevalence among the whole population; for among adults similar marks are not found to exist in a larger proportion than 1½ per cent. Among a total amount of 1521 persons, 23 presented the marks of scrofula now specified, and the author accordingly infers that among the whole population scrofula does not prevail in more than 2¼ per cent. of the people.

Among children applying to be relieved by hospitals and dispensaries, it appears that the proportion affected with scrofula is

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* Some Observations on the Tubercular or Gypseous Disease of the Lower Animals. By David Craigie, M. D., &c.

75 (this ought to be 77[*]) per 1000, or 4 per cent. of the total admissions.

Among recruits, it appears that among 95,586 examined, all of the requisite stature, 800, or 1 in 119, were rejected for marks of scrofula.

Among convicts the results are not very different. In 1840, Dr Baly found that among 1052 prisoners received at the Penitentiary at Milbank, 14 presented at the time of admission indications of scrofulous disease of the external glands. In the Parkhurst prison, Mr Jemmet found that among 660 boys between the ages of 10 and 16 inspected, 95 presented enlarged glands, and that in no instance were there scrofulous scars or diseased joints.

From these several facts Mr Phillips thinks it may be inferred that scrofulous scars are apparent in about 1½ per cent. of the population; that the subcutaneous glands are enlarged so as to be perceptible on simple inspection in less than 3 per cent., and that the glands may be detected by the finger in 24½ per cent. of the children of the poor who are under 16, and in 8 per cent. of those above; in 10 per cent. of the whole population, and that something less than 3 per cent. of the people are under treatment for the disease in its various forms.

Scrofula, as now defined by the author, is accordingly not a very mortal disease. The mortality, as given by the tables of the Registrar-General, amounts to 8 per 100,000 of the whole population of England and Wales; that is, taking the population to be 16,000,000, the average annual deaths are about 1200.

Does this, however, throw any light on the extent to which scrofula influences the mortality of other connate diseases?

It further appears that scrofula prevails very unequally in different districts; and if the circumstances on which this inequality depends could be always ascertained, it would certainly tend to elucidate the formation and generation of the disease. While the general result of the cases presented at hospitals and dispensaries is, that 1.2 per cent. are registered as scrofula, it appears that in some places the proportion is below 1; in Exeter it is 2 per cent., and in Glasgow 5 per cent. In the eastern agricultural districts, comprising Essex, Suffolk, and Norfolk, the deaths from scrofula in every 100,000 are as 11.8; and in the south-western, including Wilts, Devon, Dorset, Somerset, and Cornwall, as 10 in every 100,000, being a difference of nearly 20 per cent. In the manufacturing districts of Lancashire and Cheshire,

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		Scrofula.
Admissions in physicians' books, .	1000	19
Admissions in surgeons' books, . .	2000	212
		3)231(77

the proportion is only as 5.6 in 100,000, while in Yorkshire it is 3.5 per 100,000. In the mixed districts of Gloucester, Hereford, Shropshire, Worcester, Stafford, and Warwick, the rate is as 7.9 to 100,000.

In the Irish returns, the average is 1 in 25,952 of the population, and it ranges from 1 in 21,177 to 1 in 31,399.

As to the results from recruits, while in England the small number of 5 per 1000 took place in London, in the Edinburgh district in Scotland it is 16 per 1000, and in Dublin 11 per 1000.

Mr Phillips finds, on surveying the returns made as to the prevalence of scrofula in other countries, that in Russia it is very prevalent; that in America the statements vary a good deal, the mortality from scrofula in 1840 in New York being 1 in 1241, and in Philadelphia in 453; but much more considerable than in this country, the proportion in London for the same year being 1 in 17,500.

From examinations of children made at Beyrout, Cairo, Alexandria, and Athens, it appears that in Syria and Egypt the disease is less prevalent than in England, but more frequently proceeds to suppuration, and that in Greece it is greatly more prevalent than among the natives of the British islands.

As to India, it appears that the disease is rather prevalent. Among 100 boys under 10, all born in India, and of Creole extraction, of whom 80 may be called dark in complexion, the majority are subject to glandular disease; of 18 with flaxen hair and olive eyes, none are free from glandular tumours, but not suppurating; and two very fair, with grey eyes, are also scrofulous. Among 75 children, of mixed parentage, examined by another gentleman, all had swelled cervical glands; four had open scrofulous sores. Among 136 of pure English parentage, none were scrofulous; and among 504 native children, 300 were scrofulous. It appears, therefore, that scrofula is more prevalent among the mixed than among the pure breeds.

At Madeira, according to Dr Renton, it appears that among 405 children examined, 53 had sensibly enlarged glands, three suppurating glands, and four had scars; in all 15 per cent. This is as high a rate as is found among the boys of the Parkhurst prison.

In Paris, the deaths from scrofula amount to 1 in 3221 of the population; in Geneva to 1 in 2790; and in 1842, taking the population at 61,871, and the deaths from scrofula at 16, the proportion is 1 in 3867. In London, during the four years ending in 1842, they were only 1 in 9000.

As to the whole kingdom of France, the only data are the returns of rejections among recruits. It appears that 54,668 were rejected for all causes in a number of 86,000. This is nearly 54 in 86 persons, which is greatly higher than our own, which is only 1 in 4. The rejections for scrofula were 1754 in 86,000, or about 2 per cent., our own rates being 1 in 119.

From all these facts Mr Phillips draws the conclusion that the opinion that scrofula is an English disease is erroneous, and that there is no country, so far as our information hitherto has gone, where the inhabitants are more free from scrofula than England and Wales.

Mr Phillips next considers the question, whether the occurrence of scrofula was more prevalent at former periods than at present in this country, or is it on the increase or decrease. In order to throw some light on this question, he states in the text a series of facts, which are given in an abbreviated form in the following table :—

Period.	Population.	General Mortality.	Consumption.	Scrofula.
1700	665,200	20,900=1 in 31	3589=1 in 182	73=1 in 9,180
1750	654,000	25,350=1 in 26	4530=1 in 144	22=1 in 29,727
1801	777,000	19,680=1 in 40	5028=1 in 154	5=1 in 155,400
1811	888,000	18,575=1 in 48	4511=1 in 196	5=1 in 177,600
1821	1050,000	19,056=1 in 55	4491=1 in 233	10=1 in 105,000
1831	1233,000	20,910=1 in 61	4735=1 in 258	9=1 in 135,888

From the numbers here given, the following conclusions may be deduced. *First*, In the course of the period between 1700 and 1831, the general mortality has diminished or has been reduced about one-half. *Secondly*, The mortality by consumption has been increased fully one-third. And, *thirdly*, The mortality from scrofula has been reduced or has diminished fifteen times lower than at the beginning of the period.

This table further suggests several interesting reflections. The diminution of scrofula during a period of 130 years, if reliance can be placed at all on numerical statements, is so great, so steady, and so progressive, that it seems natural to infer that the disease may at some future period become entirely extinct. It is indeed difficult to imagine that if the diminution advances at the same rate during the next 130 years at which it has proceeded during the last space, the disease may in 1961 be 30 times less prevalent, or even may vanish to an extremely small rate.

The causes of this steady and great decrease in the prevalence and mortality of scrofulous diseases form a natural subject of inquiry. But all this is involved in great obscurity and uncertainty. Only two observations may be made as deserving attention. The diminution now mentioned it seems reasonable to ascribe in a very great degree to the improved state of the great mass of the people as to food, clothing, house-accommodation, and similar agents. At the commencement of the present century another cause was added. Vaccination, by contracting the dominion of small-pox, certainly has tended much to the same result.

The etiology of the disease, which is the great question, theoretical and practical, next demands attention.

On the first alleged cause, that of hereditary influence, various authorities, but no distinct unequivocal facts, are adduced; and unhappily it has been a subject on which authorities rather than facts have been appealed to. All this, therefore, as well as the discussion on the mode in which the hereditary influence is transmitted, we omit. We shall merely give the reasonings and facts brought forward by the author himself, who on this subject expresses himself in the following manner:—

"I now propose to consider, *first*, whether any disease existing in one or both parents at the time of the connexion from which the pregnancy resulted, or, in as far as concerns the mother, during any portion of the uterine life of the fœtus, is manifested in the child, at or soon after the moment of birth. *Secondly*, whether, under similar circumstances, the disease, though not presented in the child, at or soon after birth, may be manifested at an after period of life, and this in either event, in a larger proportion of cases than in the children of parents not thus afflicted. Upon those two questions, according to my views, the subject of hereditary disease hinges.

"There are, however, other questions, very nearly connected with hereditary influence which we must not overlook, viz.—Supposing the father or mother to suffer from any particular constitutional disease, is there any proof that their offspring are likely to become the victims of particular diseases, unlike those of their parents? In other words, whether any disease, other than scrofula, existing in either parent, tends to develop scrofula in the child?

"Have we reason to believe, that when parents are otherwise healthy, conception occurring at a particular period, that of menstruation, for instance, is likely to occasion any disease in the child resulting from that conception?

"Is there any reason to conclude, that the relative age or strength of the parents exercises any influence in determining particular diseases in their offspring?

"Have we any proof that intermarriages tend to the development of scrofula in the child?

"All those questions have reference to the influence supposed to be exerted by the parent upon the child; but there is another question intimately connected with them, namely,

"Have we sufficient reason to conclude, that a child suckled by a person, either its mother or its foster-mother, may with the milk it takes in from the breast of its nurse, imbibe the germs of any particular constitutional disease, such as scrofula, which may pervade the system of that mother or foster-mother?

"Have we then positive proof, that any constitutional disease affecting either parent at the moment of the connexion from which conception has resulted, or the mother, during any period of intra-uterine life, is manifested in the child at the moment of birth, or soon after? I think it must be admitted, that both in syphilis and in small pox, this may happen with sufficient frequency to prove that the child can in this way inherit the disease by which the constitution of the mother is tainted. And in syphilis, it is a question if it may not happen, whether the tainted parent be the father or the mother. Although difficulties often occur in verifying the truth of the statements made by parties under such circumstances, 'Yet,' says Lallemand, 'when I see a father and mother, who have not at present any symptom of syphilis, the father having previously suffered from it, give birth to four children, who all died from the effects of constitutional syphilis; when I see a fifth covered with pustular syphilis, and infecting two nurses; when I cure this child with sublimate baths and the mercurial treatment; and lastly, when after an anti-venereal treatment, administered to the parents, I see four children born to them perfectly healthy, how can I refrain from admitting that the virus has existed in the spermatic fluid of the father, and that it has passed from the child to the nurse?' It is under these circumstances, when the disease is transmitted through the spermatic fluid of the father, or the blood of the mother, that the nutrition of the fœtus is so seriously compromised; and if it do not determine abortion, the child comes into the world offering all the characters of age and decrepitude, and dies in the last stage of marasmus. Some persons conceive that this stain may be impressed on succeeding generations; though it may be manifested under different conditions, scrofulous or otherwise. Hahnemann, and others, appear to think the stain is indelible, or that for many generations the poison is felt, and that by a long mixture of races only can the last traces of the evil be extinguished.

"With respect, then, to syphilis, I think that, in the cases of Dubois and others, there is proof that the constitution of the infant may be affected *in utero*, that the disease may be apparent in the child *at the moment of birth*, or may be manifested after months have intervened. But it is impossible to explain how the possession of this taint is consistent with the attributes of high health frequently presented by the child at the time of birth. With the exception of syphilis and small-pox, I know of no disease in which corresponding evidence of such transmission can be adduced.

"Even in reference to insanity, the case of all others in which the truth of the hereditary influence might be most easily tested, and in which the conviction of its existence is perhaps the strongest, I know no conclusive evidence. A recent writer on the subject, Baillarger, assumes it to be made out; for he says, 'All agree about hereditary influence in the production of insanity.' There is scarcely a medical man in charge of a lunatic establishment, who has not a conviction of the hereditary transmission of insanity. Esquirol, for instance, says, 'Hereditary influence is the most ordinary predisposing cause of insanity;' and yet in his table of 466 cases, at Salpêtrière, it is assigned as a cause in only 105 instances. I may add, that Rush and others deny its influence. Of 191 patients admitted into Bethlehem Hospital, in 1844, an hereditary cause could only be discovered in 26 cases. And of 14,362 cases mentioned in the work of Devay, 1682 only were presumed to be hereditary.

"Those who regard cancer and insanity as hereditary diseases, do not maintain that they must be manifested at, or even soon after, the moment of birth. With respect to phthisis, persons may be found prepared to advocate the opinion that it possesses that distinctive hereditary quality of being manifested at, or even before the moment of birth, in the child of phthisical parents; but the evidence by which this opinion is supported does not appear to me satisfactory. Thus, with the exception of syphilis and small-pox, we have, then, no sufficient proof that disease existing in the parents may be expected to manifest itself in the infant at, or immediately after, the moment of birth.

"Have we then any proof that when particular diseases exist in the parents under the circumstances already stated, they will, during any period of life, be more frequently found in their offspring than in the offspring of persons not so affected? I know of no evidence bearing out that opinion, in as far as concerns insanity or cancer, though an impression exists that they will be so manifested. But with respect to phthisis, the impression is so strong as to amount to a generally received conviction.

"That tubercular depositions in the lungs have been observed at a very early period of life, is quite true. Evidence of this fact has been furnished by specimens in the museums of Langstaff and others, in which such depositions in the fœtal lungs might be seen; by the observations of Husson, (who dissected two infants, one still-born at the seventh month, the other lived eight days,—both had softened tubercles; one in the lungs, the other in the liver,) Dupuy, Andral, Œhler, (the latter of whom found the mesenteric glands tumid and scrofulous, not only in fœtuses born of scrofulous mothers, but also in those proceeding from mothers on whom no suspicion of scrofulous taint rested,) and Chaussier, (who speaks of scrofulous tumours in a state of suppuration, as well in fœtuses as in new-born children; but the condition of the parent is not noticed.) Still, even such cases are very rare. Billard only saw two or three examples, and in those instances the state of the parents was not known; Velpeau and Breschet, during their investigations in embryology, never saw an instance; and of 400 still-born children examined by Guyot, he only found tubercles in a single case, and the condition of the parent was not known. These facts, therefore, are of no use as a means of estimating how far the power exists in the parent suffering from scrofula or phthisis to transmit it to the child. Cullen speaks of a child dying scrofulous at three months; Baudelocque has seen several similar cases; but they are exceptional, and in nowise unsettle the dictum of Bertrandi, 'Raro infantes ubera sugentes scrophulosi fiunt.'

"That a sickly mother will probably give birth to a sickly child, I do not deny; the point which I do not admit is, that a scrofulous mother does ordinarily produce a scrofulous child. The best evidence we have on this point is that of Louis, and that of Rilliet and Barthez, which, though referring only to phthisis, we may fairly use in this place."—Pp. 112—117.

"Of 314 tubercular children examined by Rilliet and Barthez, the parents, or either of them, were certainly tubercular in only 25 instances, probably so in 21 more; probably, or certainly not so, in 138 instances; in 130 instances, the information was incomplete. Of 211 children non-tubercular, the parents were certainly tubercular in 12 instances, probably so in 4 more; probably, or certainly not so, in 95 instances; in 100 instances the information was uncertain. The following table contains a further analysis of these cases.

	314 tubercular.	211 not tubercular.
Hereditary influence without hygienic causes,	17	5
,, with ,,	18	6
,, with different diseases,	9	10
,, without ,,	37	6
Hereditary causes alone,	11	2
Hygienic causes without hereditary influence,	46	35
,, ,, other diseases,	61	36
,, with ,,	20	25
Hygienic causes alone,	43	18
Diseases without hereditary influence,	48	48
,, hygienic causes,	18	21
Diseases the only cause,	8	10

"It is clear that this is the only method of investigation by which the point can be determined, in respect to any hereditary disease, whether phthisis, or cancer, or scrofula, or insanity. And until this can be done on a large scale—and much time must elapse before it can—our ideas on the subject will remain as unsettled as they are at present, and a very serious social evil will be perpetuated. I do not mean to say that all men are prevented from intermarrying with females proceeding from scrofulous families; though, if it be a fact that a scrofulous mother brings forth scrofulous children, such marriage is a serious evil. Still, since a young lady will have her chance of marrying very much lessened if an impression exists that she is scrofulous herself, or comes of a scrofulous family, it is very important to seek the best evidence we can obtain for the purpose of approaching the truth, in a matter so deeply affecting society.

"The means which I have taken to acquire accurate data as to the extent to which hereditary causes operate, in the propagation of scrofula, are the following. I examined myself, and procured to be examined by others, in the metropolitan, the factory, and in rural districts, upwards of 2000 families, each consisting of from three to five children, and living, as nearly as may be, under similar circumstances. In one portion of the cases, both parents were apparently free from scrofulous taint; in another portion, there was reason to think that both parents were tainted; in another, that the father was tainted; and in another, the mother. The number of families examined was 2023, the number of children was 7587; and the number bearing such marks of scrofula as I have already indicated, was 1738, or nearly 23 per cent. In 506 instances, derived from many localities, and under the most varied circumstances, both parents were apparently untainted, and their offspring amounted to 2021. Of these, 421, or something less than 21 per cent., presented marks of scrofula. In 276 instances, there was reason to think that both parents laboured under scrofulous taint; their offspring amounted to 1092 children; of these, 271, or nearly 25 per cent., bore the ordinary marks of scrofula. In 589 instances, the father carried about him marks of having suffered from scrofula, whilst the mother was free from them; their children amounted to 2107, those having marks of scrofula to 483, or nearly 23 per cent. In 652 instances, the mother bore upon her person the marks of scrofula, whilst the father did not; their children amounted to 2367, and of these 563, or nearly 24 per cent., presented marks of scrofula.

"In glancing over those results, it must be kept in mind that the offspring of the tainted, on the one hand, and of the untainted, on the other, are not intended to represent their relative fecundity; for means were taken to collect only such families as were represented by not less than three, nor more than five children.

"It will be observed, that although an hereditary influence must be admitted to be present, and is apparent in each class, yet, at its maximum, the influence does not appear to be quite 4 per cent. It would seem that the influence of a scrofulous mother upon the offspring is greater than that of a scrofulous father."—Pp. 117—120.

The subject is unquestionably one of infinite difficulty, and involved in much natural obscurity and not a little adventitious darkness. It seems almost impossible to deny the occasional prevalence of a hereditary disposition or taint; and most physicians of any length of experience have seen too much cause to believe in the certainty of transmission in this manner. At the same time, when we consider that, according to the returns referred to in the preceding page, a gradual and steady diminution of the disease is manifestly established, it seems difficult to believe that the influence of hereditary taint is very great. We think it indeed impossible to deny it,—as easy to maintain it as to bring arguments against it; and while we conceive that it is one cause, we add that it seems not to be very powerful. Its effects are, we think, according to some observation, chiefly observed in the offspring of parents who have married rather late in life. It may be often observed, indeed, that where the father is advanced in life, the children are the subjects of strumous disease, either in the glands or the cellular tissue, or in the joints or in the periosteum. More decidedly is this the case where both parents are a little above the usual matrimonial age. Yet neither are youthful marriages unprolific in this disorder. In many instances, where married couples are unusually juvenile, the first child or two are strumous, while those born at a later period show firmer and better health. From all these rules, nevertheless, there are exceptions; and probably the subject is not susceptible of perfect accuracy.

With the question of hereditary influence that of predisposition is closely connected. Several authorities have indeed denied that diseases could be said to be hereditary, and have maintained that it is only the disposition to the disease to which this denomination can justly be applied. Such was the opinion of John Hunter, of Baudelocque, and of Hufeland. Yet on this very subject John Hunter expressed himself with more than his usual obscurity:—"There is no disease whatever that becomes constitutional, but what can be given to a child; there is no disease that is acquired, and becomes constitutional in the father, but can be given to a child. The father has a power of giving that to the child, by which it becomes hereditary. There is no such thing as hereditary diseases, but such a thing as hereditary disposition."

Notwithstanding the great authority by which this curious dogma is promulgated, it has always appeared to us as a distinction without a difference—as, in short, little better than downright nonsense. How it can be said that diseases are not hereditary, when we know and are constantly told that a mortal being —that is, a being that must die some time, and by some disease, or by several diseases, begets a mortal being to undergo a similar fate, is a thing we cannot understand? To say that the disease is not transmitted, is to say that the begetting individual is at the time in apparent health. But to say that the disposition only is transmitted, is a very nugatory and frivolous mode of getting over the difficulty. Though the individual be at the time in health, we know that he may become diseased because the textures of which his frame consists contain in their construction and composition the elements of disease. They contain, indeed, what the older pathologists called the seminal principles (*seminia*) of disease. On this subject the author makes the following remarks:—

> "I feel very strongly that the term predisposition has been employed only for the purpose of avoiding a difficulty which the hereditary theory is supposed to present, and that it offers a something still more difficult to test than the principle of hereditary transmis-

sion. If it were simply intended to maintain that a parent may bring forth a sickly or weakly child, and therefore predisposed to many diseases, it cannot be doubted that this happens; that such a child may become scrofulous is equally true, even when there is no appearance of scrofulous taint in the parent; and in such a sense the influence of predisposition may be admitted, because there can be no doubt that a weakly child, exposed to the causes of scrofula, will be more likely to become the subject of the disease, than a child who possesses more power of resistance. But I apprehend the theory of predisposition was assumed for other reasons; parents having marks of scrofula brought forth children without any similar marks, and the advocates of the theory of hereditary transmission, which required that the child should come into the world with the signs of the disease, finding it did not, abandoned the peculiar theory, and substituted for it that of predisposition; but I do not so far limit hereditary influence as to require that the disease derived from the parent should of necessity be present at the moment of birth. It is true that the difficulties we meet with in the solution of this question are much increased if the disease be not presented in the child for months or years after birth, because the influence of other causes upon the child have then to be estimated, and this is no easy matter, as is evident upon a statement of the following case:—

"Let us suppose a child to be the issue of parents in robust health, to be blessed with a strong constitution, shown by its vigour and its tone; let that child be placed under circumstances calculated to debilitate him, let him be badly fed, let him breathe impure air, let his habitation be damp, his person neglected, and the effects of these influences on his constitution will be marked enough; his person will be blighted, and tubercle and scrofulous matter will be deposited in different organs. Every observer has, unhappily, had but too frequent opportunities of witnessing such cases; and I must regard the frequency of their occurrence as proofs that the circumstances in which many persons are placed are sufficient to induce the development of scrofula. It is no proof to the contrary that children may be found who resist the action of such causes. I saw a child in the wretched old workhouse of Bethnal Green, born there, the child of misery; living under-ground, in a dark, damp, ill-ventilated room, yet presenting all the appearances of robust health. But what a contrast it exhibited to those around it! I do not doubt but that individuals do possess tendencies, or predispositions, to contract particular classes of disease; but where such tendencies give no indication of their existence, they are elements of which no use can be made in scientific inquiries.

"In the present state of our knowledge, it is impossible to say in what consists the predisposition to scrofula, or to indicate the characteristic signs, except those constituted by organic or functional changes, which show the actual existence of the disease. The predisposition to scrofula conveys, to my mind, a condition of the economy which has no external, distinct, and constant signs; although I would by no means say that its existence is not real, or that direct observation may not hereafter make it known.' —Pp. 123—125.

The tuberculous cachexia admitted by Sir James Clark is next considered; and the author sees reason to refuse to that state the influence which has been claimed for it in the production of scrofulous disease. While he does not deny that the opinion of Sir James Clark may be correct, he thinks that in its present shape that opinion is not established. This conclusion he founds on the result of examinations of a number of children under five years, by Barrier, and which furnished the following data:—

Of 166 children strongly constituted, 21 only were tuberculous, or 1 in 8; of 114 children moderately constituted, 27 were tuberculous, or nearly 1 in 4; of 99 feeble children, 45, or nearly 1 in 2, were tubercular.

	Tubercular.	Not tubercular.
Strong constitution,	47	60
Feeble,	133	4
Moderate,	94	46
Not noted,	40	101

The author infers, therefore, that a feeble constitution is a favourable though not a necessary condition to the development of the tuberculous cachexia; that a feeble constitution is more likely to be the offspring of diseased than of healthy parents; but that we cannot specify the causes in the parent which tend most to entail feebleness on the child, nor what kind of disease or feebleness in the parent tends most to induce scrofula in the child.

It is totally superfluous to give any attention to the question examined by the author, whether the presence and operation of the syphilitic poison has any influence in the production of scrofula. This doctrine has been more or less confidently maintained by a great number of writers. But apparently the grounds are exceedingly precarious and uncertain. Syphilis can in no way be said to give rise to scrofula, save by impairing the powers of the system generally, or by the deleterious effects caused by the excessive use or abuse of mercury for its cure; and indeed there is a fallacy in the whole of this reasoning, in so far, as what was supposed to be the influence of syphilis in producing scrofula, was in multitudes of cases the influence of large, long-continued, and repeated courses of mercury in inducing scrofula. The author decides against the influence of syphilis; but he seems not to be aware, or not to think, of the deleterious effects of mercurial courses.

The author thinks that there is no proof that marriages among healthy persons tend to the production of scrofula. But he allows that intermarriages may give rise to the development of various physical and mental influences of another character, and that character morbid, we are led to understand. So far as we understand his mode of stating the question, it is not whether the marriage of persons of scrofulous habit, or appearance, or disposition, may increase this habit or generate it in the offspring; but whether intermarriages of nearly the same rank or the same breed may increase the prevalence of scrofula. It is easy to see that this is a question to which, like several others in this inquiry, it is impossible to give a positive answer. We think it impossible to doubt, that if two persons of slight scrofulous habit marry, their offspring must, according both to physical and physiological rules, have a chance, if not a certainty, of possessing a double share of the scrofulous habit. On the other question, whether intermarriages among the same order, rank, tribe, or cast, have influence in this way, the author appeals to the society of friends, who marry almost entirely among their own sect; to the nation of the Jews, who also marry much among themselves, unless when they wish to catch some Christian; and to the royal and noble families of European nations, who in like manner marry much among themselves, to show that this practice does not tend particularly or strongly to the production of scrofula.

The influence of the milk of the nurse might be supposed to be both direct and considerable. Yet the author seems to doubt this influence altogether. After observing that we know nothing of the chemical characters of scrofulous milk, he makes the following observations:—

> "I have not succeeded in procuring any human milk, which yielded anything like a similar difference; but then those specimens which I have been able to procure were not taken from persons actually suffering from scrofula, but from persons who presented marks of the previous ravages of the disease. In the absence of experimental proof, analogy would fully warrant the assumption, that changes are produced in the secretion of milk, as well as in the other products of the human economy. In a highly scrofulous patient, most of the secretions seem to be preternaturally acid; the cutaneous transpiration, the urine, the fœcal matter, and purulent fluid, all exhibit that character, frequently in considerable intensity. The same character is detected in the odour of the breath; but what influence milk so changed will have upon the child is not shown. A mother does not knowingly choose a scrofulous wet-nurse, because such a person is unhealthy, and because her milk will of course be so also. If a mother suckling her own child were scrofulous, and the child were to become so, three things would have to be decided; and these in a presumed case surrounded with infinite difficulty, namely: was it hereditary—was it acquired with the milk—or was it owing to other circumstances? Although I adhere to the opinion I have expressed, that scrofula is not to be contracted at the nurse's breast, I should be quite as unwilling as

any one could be to have a child suckled by a scrofulous nurse. At the same time then, that as I conceive there is no proof in support of the opinion, that the disease may be propagated in this way, neither is it easy to procure proof that it could not happen, since such proof could only be negative; meanwhile, as all our present evidence is negative, we are justified in saying that such communication is, in the present state of our knowledge, inadmissible."—P. 142.

The subjects of contagion and inoculation of the disease by purulent matter may be passed over as insignificant. Under the latter, indeed, the author has introduced the influence of small-pox; we think not very logically. He quotes the authority of Rilliet and Barthez to show that a greater number of vaccinated children die with, than without tubercles, though not proving that vaccination causes tubercular deposition. Little precise or satisfactory information is here communicated; and the point the author leaves much where he found it. We cannot quit the subject without saying, that multiplied experience has long shown that small-pox is a most powerful exciting cause of the strumous disposition; that in the strumous that disease is always most severe, and causes most injurious effects; and that we think one main cause of the diminution of scrofula, as shown by the table referred to in page 131, in the first ten years of the present century, appears to be attributable to the favourable influence exerted by vaccination in counteracting and diminishing the prevalence and virulence of the variolous poison.

The author next considers the influence of particular agents, as food, mode of life, air, confinement and liberty, occupation, and similar circumstances.

This inquiry he commences by remarking that the respective mortality of any two or three towns or districts is not in all cases a just representation or index of the actual salubrity or insalubrity of the situations. Other causes, as habits and modes of life, operate very powerfully to modify the amount of disease and mortality; and of this he adduces, from a paper by Dr Guy, a very good illustration. Among the peerage,—a class including the greatest number of physical advantages, and having at its command all the means by which health may be preserved and the best advice procured in case of accident or illness, the expectation of life at the different ages below specified is represented by the following numbers, as contrasted with the general population:

Year.	Per centage.	General Population.
At 20	38.47	40.69
30	30.87	34.09
40	24.45	27.47
50	17.92	20.84
60	12,56	14.58

Thus at every age, the individuals composing the general population have a better and longer tenure of life than the members of the favoured class. This difference, these peculiarities the author ascribes to social position; upon which we suppose it is superfluous to give any comment, as the author gives none. If any one is in difficulty, he will find it given by Swift long ago in Gulliver's Travels.

A supply of proper food and fresh air with good clothing are manifestly the great means of preserving health at all periods of life, and especially in infancy, and warding off the approaches of disease and especially scrofula. The conviction of the truth of this proposition leads the author to give a detailed examination of the results of inquiry as to the influence of different kinds of food and different modes of living in giving rise to scrofula, or preventing the development of that disease. He considers first, the food of infancy and the different forms in which it is presented, the circumstances by which it is modified, and the mode in which digestion is liable to become disordered, and nutrition accordingly to be impaired or prevented.

" When a child is brought up by hand, there are two material circumstances to be noted, the food he takes, and the manner in which it is taken. The food, even if it be milk, (instead of being taken at the moment from his mother,) has probably been obtained, some time before, from a purely herbivorous animal, the cow, and between that fluid and human milk the differences are considerable, as will appear from the following analyses.[*]

" The great difference in those results shows that the composition of the fluid is very variable, but in one respect they concur in showing that the milk which most resembles that of woman is that of the ass, which is obtained by very few children. That of the cow contains more than twice as much caseine, and less butter and sugar of milk, so that ordinary dilution cannot make its composition to resemble that of the milk of woman. But there is a further difference which cannot be unimportant. If an infant be suckled by two different persons, his digestion is likely to become deranged, and yet the food taken by the child, brought up by hand, may be derived

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	Cow.	Ass.	Woman.	Goat.	Ewe.
[*] Caseine, . .	4.48	1.82	1.52	4.02	4.50
Butter, . .	3.13	0.11	3.55	3.32	4.20
Sugar of milk, .	4.77	6.08	6.50	5.28	5.00
Various salts, . .	0.60	0.34	0.45	0.58	0.68
Water, . .	87.02	91.65	87.98	86.80	85.62

Henri et Chevallier, Journal de Pharmacie, t. xxv., p. 340.

	Cow.	Ass.	Woman.	Goat.
Caseous matter, . .	8.95	1.95	1.93	4 38
Butter, , .	2.68	1.20	8.97	4.56
Sugar of milk, . .	5.68	6.29	1.20	9.12
Water, . . .	84.69	90.95	87.90	81.94

Meggenhofen, Van Stiptrian Luiscius Compt. Peligot.

from the mixture of the milk of many cows. Again, when he derives his food from his mother's breast, it is of a proper temperature and homogeneous; when obtained from the cow, it may have been drawn so long as to have become separated into two parts, or to be even further decomposed. How often is the influence of these things apparent; a child loses its mother, an attempt is made to bring it up on cow's milk, it gradually droops and languishes; a foster-mother is then procured, and it rapidly recovers. Again, even if the milk of a healthy cow were unexceptionable food for the child, it happens, particularly in large towns, where the animal is kept in a state of close confinement, that she becomes liable to disease of a tubercular nature, and her milk undergoes considerable change in its composition; acquires a sweeter taste than is natural, and the proportion of its saline elements is much increased. In this state, it is said to be less easily digested, more apt to disagree with the delicate stomach of the infant, and to impair nutrition.

" Then the mode of taking the food exercises an important influence upon the health of the child. By the act of sucking, a certain quantity of saliva is pressed into the mouth, and is mixed with the milk so as to render its digestion easier. Indeed the admixture of saliva with the food is an essential condition of a good digestion, not only in children but in grown up persons. Add to all this, that the natural mode of taking food is to some extent a protection against improper feeding. The act of sucking is an exertion which can only be maintained for a certain time, and therefore the chance of improperly distending the stomach is not great; but when fed by hand, no such exertion is necessary, and the only measure of the capacity of the child's stomach and of its wants, is the judgment of the nurse. And the chance of harm from over-feeding is thus considerably increased.

" If these evils are attendant upon bringing up by hand, even when milk is used, they are unquestionably greater where the food is of a different kind. In the great bulk of cases, instead of milk, vegetable substances are used; gruel, with a little milk, sopped bread, or flour, or other farinaceous substance. The delicate mucous surface of a child is at first only fitted for the conversion of such food as is very easily assimilated, and the breast of the mother furnishes such a pabulum; but the ordinary farinaceous food is difficult of assimilation, and the digestive surface becomes so far deranged as to unfit it for the purpose of healthy nutrition. The evils upon which I have dwelt are made evident by the following facts. In Lancashire and the West Riding of York, the deaths in the first year of life, are, to the total deaths, as 1 to 3.9; while in Devon and Wilts, they are as 1 to 6.4! Now it is in the great factory towns of this country, which are found in Lancashire, Cheshire, and Yorkshire, that the system of bringing up the child by hand is most commonly practised, and where its evil effects are most apparent; first, as we have seen, in the great destruction of infant life, and failing that, in the development of scrofula. It is not that the mother has no milk, but that in such places she is enabled to make what she considers to be a more profitable use of her time than by staying at home and nursing her child.* Her child may be suckled at early morn, and again in the evening, possibly, too, at the middle of the day; but whatever food it may require at the intervening periods, if furnished at all, is afforded in the shape of the crudest and most inappropriate substances, and restlessness is known in many places to be habitually repressed by Godfrey's cordial.

"The digestion of infants is very rapid, and as the quantity of food taken at a time is small, it is necessary that feeding should be frequently repeated, and that during the first month the interval of feeding should not exceed from one and a half to three hours, because if longer periods are allowed to intervene, nutrition languishes, disease is developed, and life is ultimately sacrificed. The proof of the statement I have made, that bringing up by hand tends to produce scrofula, is to be found in the larger factory towns, where during the first year of life, the deaths from tuberculous and scrofulous diseases are as 1 to 31 of the total deaths, whilst in the metropolis they are as 1 to 42. During the whole of life they are 1 to 5.6 in the factory districts, and 1 to 6.4 in the metropolis. The facts I have adduced prove, as I submit, that because the food itself is not appropriate, and the mode of taking it is not natural, it acts injuriously upon nutrition, and that a larger proportion of scrofulous disease is found among children who have been brought up by hand than among those who are suckled by a mother, or even a foster-mother; and that therefore the system of bringing up children by hand deranges nutrition, and adapts the constitution for the deposit of scrofulous matter in the child."—Pp. 161—163.

If this be the influence of improper feeding in infancy, there can be little doubt of food exerting the same influence in childhood and after life, though perhaps it is at the former period of life more decidedly efficient. All good observers indeed have agreed in the general fact, that where food is scanty and accordingly bad and innutritious, for these two qualities are invariably associated, the race becomes sickly and liable to disease; and when liable to disease, scrofula is as likely to be generated as any other.

The facts which the author has collected on the influence of food in union workhouses and similar establishments for the poorer classes of our countrymen are of great value not only in illustrating his own subject, but that of the influence of different sorts of food in general in the maintenance of health or the development of disease. The following statements show, first, the general mode of feeding in the workhouses.

"The food on which the inmate of the Union House subsists is,

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* Thus, in the Cotton Districts supervised by Mr Horner, 141,450 persons above eighteen are employed; of these, 77,208, or 54½ per cent. are women.

I conclude, represented by one of the following Dietaries. Of the six Dietaries contained in the Poor Law Circular, I extract the highest and the lowest; in the former, every able-bodied pauper gets weekly, 116 ounces of bread, 6 pints of soup, 18 ounces of meat, besides 10½ pints of gruel, 24 ounces of suet pudding, and 14 ounces of cheese; in the latter, 102 ounces of bread, 8 ounces of meat, 2 pints of broth, 13 ounces of cheese, 38 ounces of yeast dumpling and suet pudding, and 24 ounces of potatoes. And for children under nine years old, the common direction is, that they are to be dieted at discretion.

"The dietary for children above nine is, as nearly as may be, the following. Highest, 70 ounces of bread, 10½ pints of gruel, 15 ounces of meat, 4½ pints of soup, 4½ pints of broth, 12 ounces of suet pudding, 1½ lbs. of potatoes, 8 ounces of cheese. Lowest, 98 ounces of bread, 14 ounces of cheese, 10 ounces of meat pudding, 20 ounces of suet pudding."—P. 169.

Then come the food and maintenance of the independent labourer.

"The common estimate among the peasantry is for bread, 1 stone of flour for each grown person, half a stone for each child, a-week. It is, however, clear that this must be subject to great variations; I am inclined to think it the minimum, where there are sufficient means. Butchers' meat may be said not to enter at all into consumption as an article of food. A part of a pig, when they can afford to keep it, is a luxury; 2 or 3 stones reserved after harvest is, I think, all that the most prosperous in the parish have done. Butter is here constantly bought, a pint when it can be afforded, half a pint always, if possible; 1 pound of cheese may also be set down as a regular weekly supply. I enclose you a couple of papers, containing a very exact account of the gains and expenditure during the past year, of two large families in the parish; and the mode of existence among the peasantry is so similar, that they may be taken as a general view of the subject.

Family: man and wife; 4 girls, from six to twelve; 1 boy, three.

Total income, L.31 4 0	Rent, . . .	L.2	10	0
	Rent of ground, .	0	11	0
	Shoe bill, . . .	3	3	6
	Club, . . .	0	12	3
	Children's clothes, .	1	1	0
	Weekly expenses, as below,	21	13	3
		L.29	11	0

WEEKLY.

3½ stone of flour, 1s. 8d. .	L.0	5	10
Yeast and milk for baking,	0	0	3
½ lb. candles, . .	0	0	3¼
½ lb. soap, . . .	0	0	3
Tea 2d. sugar 3½d. .	0	0	5½
Butter, 6d. cheese, 3d. .	0	0	9
Sundries, .	0	0	6
	L.0	8	3¾

Man and wife and 6 children.

Total gains, L.34 2 8	Rent of house, .	L.2	10	0
	Rent of ground, . .	0	11	0
	Shoe bill, . .	3	5	0
	Tools, . . .	0	17	6
	Weekly expenses, .	24	18	3
		L.32	1	9

WEEKLY.			
3¾ stone of flour, 1s. 8d.	L.0	6	3
Tea, 4d. soda, 1d. soap, 3d.	0	0	8
1 pint butter, 1s. 1 lb. cheese, 7d.	0	1	7
Sugar, 3½d. candles, 3¼d.	0	0	6¾
Sundries, . .	0	0	6
	L.0	9	6¾

Nothing for milk, coals, bacon, meat, clothes, (except shoes.)"—Pp. 170—171.

From this it appears that the inhabitants of the Unions are much better off as to diet and sustenance than the independent labourer; and indeed it is said by Mr Howman, one of the authorities quoted by Mr Phillips, that no independent labourer can obtain the diet which is given in the Union Workhouse. This is no doubt a melancholy truth; and it is matter much to be deplored, that hard working men with their wives and families cannot earn so much, as to supply them with proper food and clothing and shelter; and that even in the Unions, where the food is more abundant, it is nevertheless not of the kind best calculated to maintain health and strength at the same time. What can be a worse or more hurtful species of diet, always excepting the bones of the Andover Union, to some of these people than the quantities of cheese and suet dumpling which they are obliged to consume, in order to satisfy the cravings of nature. Yet of these the independent labourer would probably be glad.

Next then we come to consider the relation between the mode of feeding and the prevalence of scrofula.

"To ascertain," says Mr Phillips, "how far scrofula prevails in Union Houses, 9342 children, the inmates of 63 houses, in different districts, were examined; and of these 2139, or nearly 23 per cent., presented marks of scrofula.

"In a large town, I examined 784 children in endowed and other schools, who for the most part had been well cared for before admission, and judiciously managed afterwards; and the "marks of scrofula" could be detected in 127 instances, or 16 per cent.; 500 children in National Schools, brought up at home, of whom the most part were not well fed and lodged, and of these 164 or 32 per cent. bore similar marks of scrofula. And 500 children examined in Workhouses, yielded 126 or 25 per cent. bearing marks of scrofula.

" If we compare the children in the St. Marylebone Workhouse with the children of the National School in High Street, St. Marylebone, there will be no question of the physical superiority of the former, for while the children in the Workhouse are affected with scrofulous swellings to the extent of 26½ per cent. only, those of the National School are affected in the proportion of 32 per cent.

" In the same districts with those of the 63 Union Houses, 22,704 children living at their own homes, or in institutions other than Poor Houses, have been examined; of those 8353, or 37½ per cent. had marks of scrofula.

" The dietaries of Union Workhouses, as contained in the Poor Law Circular, having been compared with the food of the agricultural labourer, and the prevalence of scrofula in each condition having been shown, we arrive at this result, namely, that the Workhouse child is better fed, and less subject to scrofula than the child reared in the cottage of the peasant; and when it is considered that many pauper children are received into the Workhouse when suffering and destitution have probably developed disease, and have almost certainly produced debility, the comparison is even less favourable for the child of the independent labourer, than the result which is shown by the numbers actually examined. Now with the exception of food and clothing, the Workhouse child enjoys no advantages of which the child reared in the cottage is deprived. The rooms he inhabits may be larger and better ventilated, but a greater number of persons is collected in those rooms, and the breathing space for each person may even be less than in the narrow limits of the cottage."—Pp. 171—173.

From all these facts the author deduces the inference, that the pauper children require for the maintenance of health and strength and the repulsion of disease a still more liberal diet than they at present possess; and that this would be more consistent with sound national economy.

If this be requisite for paupers, what is required for the children of the independent self-supported labouring poor? The author admits that the workhouse child is less subject to scrofula than the children of the independent labourer; and if, notwithstanding this superiority and advantage, the author thinks the workhouse children should have better and more nutritious food, how much more ought the children of the peasantry to be provided with more abundant and more nutritious food? The fact seems to be certain, that immense numbers of the labouring poor, especially in the agricultural counties, are so thoroughly steeped in the most hopeless and grinding poverty, that during the whole twelve months of the year they live at what may be termed starvation point; and yet with all this privation they seem to enjoy a very reasonable amount of health.

The counteracting remedy is in many instances the living much in the open air, the sense of freedom, and the consciousness that they are not immured within the precincts of the workhouse. This part of the inference is forcibly illustrated by the effects of confinement in prisons, in which great numbers of persons become feeble, diseased, and not unusually die. This is shown by the returns of the Milbank penitentiary, which appears ever since its construction to have been an institution remarkable for its powers in producing disease. The readers of this Journal know what took place within that building in 1821 and 1822; and though many precautionary measures as to diet, ventilation, warming, and internal regulations, have been since adopted, it is certain that many of the penitents continue to be attacked with scrofula, consumption, and other complaints, the effect, in some degree, apparently, of confinement; that not a few die in consequence; and that others receive pardons previous to the termination of their period of confinement. The details showing this are given in a valuable letter from Dr Baly, which our limits will not allow us to introduce here, but the principal facts of which we hope to be able to give in the Medical Intelligence. They are quite conclusive as to the noxious effects of confinement. Unquestionably the limited diet of many prisons has also a hurtful effect; and Dr Baly makes the observation, that "by far the thinnest convicts, and those having the largest proportion of unhealthy and scrofulous individuals among their number, come from the Scottish prisons, in which the diet consists of a sparing allowance of vegetable and farinaceous food."

We know well that various examples occur, from time to time, of persons attacked with illness after confinement in the prisons of this country. But the diseases most prevalent are bronchitis, pneumonia, and consumption; and these seem to be the result, most frequently, of confinement, ill-aired, cold chambers, and exposure to cold generally, with inaction of the muscular system.

The general conclusion deduced by the author is, that in Great Britain scrofula is least prevalent, where children and others are best fed; and, though he does not assume that the immunity is owing to superior feeding, because other accessory agents have likewise great influence, he thinks it a just deduction from the facts, that food exercises a more important influence than any other agent in the production of scrofula.

We have seen that the food of the peasantry and labouring agricultural population is scanty in quantity and inferior in quality. Yet we are assured that it was at one time, about fifty years back, still worse; and that it has undergone, during the last forty or fifty years, a very great amelioration. The author states that, within his own recollection, barley bread was the food of a large part of the labouring poor in certain districts in England; but now, in the peasant's cottage, good wheaten bread is everywhere found. The barley bread we do not regard as the worst of food, if enough of it can be procured; and we think that, wherever the grain meal, as barleymeal, peas, or oatmeal can be procured,—though it is always desirable to get wheaten bread,—still the labourer may be well fed on the former. It is only where the cereal grains cannot be obtained in sufficient quantity that food becomes scanty and innutritious.

The results obtained from the examination of children of charitable institutions in foreign countries, compared with the dietaries of these institutions, lead to similar results.

The purity or impurity of the atmosphere is a very powerful agent in the prevention and the production of disease; and not less so appear to be the changes of which the atmosphere is the medium, in its powers of producing heat or cold, dryness, or humidity.

Though it appears to be established as a general fact that the mortality of manufacturing is higher than that of rural districts, yet it appears that the mortality of all town populations is not equally high. From the reports of the Registrar General, it results that, among twenty-five towns, excluding the metropolis, and possessing a population of 1,883,693 souls, the deaths amount to 2.733 per cent; while in rural districts, with a population of 3.440,501, the deaths amount to 1.906 per cent. The mortality of the country districts and the towns, when the proper correction is made for the difference of the numbers of the population, are as follow:

	Country districts.	Town districts.
Deaths in the same time out of the same population,	132,116	185,966

The excess of deaths in the town population is 53,850.

It appears that the difference as to the excess of deaths by scrofula, in country districts above that in towns, according to Mr Farr's calculation, is 101 above 52,—nearly 2 to 1.

It appears, further, that those who, it might be imagined, are, from wealth, easy circumstances, and other means, placed above want, or privation of any kind, by no means appear to have the primest or longest tenure of life. Mr Phillips adduces, from a paper by Mr Neison, various facts which show that the hardworking and industrious members of society, though fed on the coarsest materials, and sometimes not very abundantly, are in this respect in a superior condition to the wealthy and affluent.

> "The circumstances in which the humble and working population of the country is placed, have generally been thought adverse to a prolonged duration of life; but the healthiest life tables hitherto formed, have not shown any thing so favourable as the present re-

sults, deduced from the operations of friendly societies, even among what are generally considered the select classes of society,—

" 'It may be well to be understood here, that the persons composing friendly societies, are almost exclusively the hard-working members of the community, chiefly occupied in the drudgeries and toils of the mechanic arts, and consequently exposed to the inclemencies of seasons, excesses of temperature, impure atmospheres, constrained postures, and other conditions, usually thought objectionable. Their incomes are very limited, affording but the scantiest and simplest means of support. Their habitations are of an inferior order, being of the cheapest kind, and consequently in the worst streets. The members of friendly societies are, therefore, generally placed in those circumstances which persons habituated to the luxuries of the upper ranks of society would regard as unfavourable to health and to a superior duration of life. In making these remarks, however, it is necessary, as will hereafter be seen, to make a distinction between them and the great bulk of the poorer classes of the country. For an individual to remain a member of a friendly society, it is required that he should make his weekly or monthly contribution to its funds; and although a few pence is all that is needed, it presumes a certain amount of frugality and industrial habit, sufficient to separate him from the reckless and improvident person, who is more openly exposed to the vicissitudes—poverty, distress, destitution, and disease—incidental to the fluctuations in the demand for labour.

" 'The superior value of life among the members of friendly societies, is a very remarkable and important feature in this inquiry, and is a result that generally would not have been anticipated; and the question which naturally follows, is, From what source or class does the excess of mortality, which makes up the general average of the community, arise?

" 'Those persons having transactions with assurance companies, belong, with a few exceptions, to the middle or higher ranks of society; and if the value of life, as deduced from observations in those companies, be admitted as a correct measure for such classes, it will be found that their duration of life is not only less than among the members of friendly societies, but also less than in the country generally.

" 'It may be said, in reply to some of the preceding observations, that the superior value of life in the ranks of friendly societies, above the general community, is owing to the effects of selection; but a little reflection will show that the difference must be produced by other causes. Every reasonable means is adopted to test the lives admitted into assurance companies, and yet they appear to be of less value than the general average of the country; and friendly societies are known *not* to exercise the same degree of scrutiny. In both, the interest of the applicant for admission is opposed to that of the society; and, looking at the results, it is not unlikely that the vigilance of the one may be neutralized by the interests of the other. Another result, brought out by the observations on the lives in assurance offices, will show how inadequate the means of selection usually resorted to are, to raise the standard of life above the average of the country. All other inquiries hitherto made on male and female life, have tended to attach a greater value to the latter than to the former; but the results in the assurance companies have been reversed, showing that some other causes, beyond the method of selection, must have interfered to modify the state of health; for if the means of scrutiny had been adequate to determine the actual character and condition of health, the prevailing feature of each sex would have manifested itself, and the anomalous result of male life being of higher absolute value than female life would not have appeared.

"Assurance companies, it has been stated, are likely to have proposals most freely from among unhealthy persons for sums payable at death; but that proposals for annuities, or sums payable during life, will be made on the lives of the most healthy only; and that the private opinion of the individual being always brought to bear against the company, the effects of selection, under this aspect, ought to prevent the results of such observations from being regarded as a true exponent of the value of life in the class of society generally to which those persons belong. There exists no published document, so far as assurance offices are concerned, to show whether this opinion is well founded; but there is evidence of the same kind—of equal, or perhaps, from its greater extent, of higher value, than any to be drawn from the assurance companies. The tables calculated by Mr Finlaison, on the lives among the nominees of the government tontines and annuity schemes, are here alluded to. The facts over which his observations extended, possessed almost every advantage that could be desired; and, considering the acknowledged skill and care with which his computations were managed, the government table must be entitled to the highest confidence, and the expectation of life thence deduced regarded as the true measure of life, in that particular class of society.

"'From a comparison of these data, then it follows that the male lives selected for the government annuities, are not only of less duration than those of the male population of the country generally, and also of less value than lives in assurance companies, but they are actually of less value than those of the members of friendly societies in the city districts. It is evident from these results, that the presumed power of the individual to judge of his own state of health has not shown the remarkable effects anticipated; there is more reason to believe that the natural inclination with which every person is led to look upon his life as good, will very much influence any power of discrimination on his own chances of longevity. It is, however, to be kept in view, that persons of decidedly bad health will rarely purchase annuities; and the exclusion of these has, no doubt, some effect in slightly raising the standard of the table. A similar observation is also made with respect to the applicants to assurance companies. There is a strong temptation for those in really bad, or at least in indifferent health, to offer themselves for assurance; and if all were admitted, no doubt a lower value would be expressed by the tables. The known caution, however, usually exercised in these matters, and the medical talent brought to the aid of the offices, is a protection against the very worst lives of that class; about 23 per cent. of the applicants being the average number rejected.

"'Friendly societies, although not equally solicitous, are still not without tests for the admission of members, and they possess one advantage over assurance companies; the members, and those likely to be candidates, are generally intimately known in their daily habits and ordinary health to each other; and where evidently bad health exists, admission is refused. A consideration of all that has been advanced, will show that the greater vitality among members of friendly societies cannot be accounted for by the superior mode of selecting lives; for, if that argument were carried out to its full extent, it would go to prove that the other classes in question had, in that respect, the advantage. The blessing thus bestowed on the frugal and industrious workmen of the country, composing friendly societies, in having granted them, as appears by the present inquiry, a prolonged duration of life, must therefore be regarded as a really true and distinctive feature of that class of persons, and is no doubt the result of their simple and uniform habits of life, and the more regular and natural physical exercises to which they are habituated.

"'Perhaps no statistical facts are better established, than the duration of life among the middle and upper classes of this country; and if the data brought forward in this paper be received as of sufficient merit to represent the duration of life among the working classes, it will then appear clear that any important change to be hoped for in the value of life, in the town districts, must be effected through other means than sanatory regulations.

"'Those persons purchasing government annuities, and having dealings with assurance companies, are certainly beyond the reach of any improvements to be introduced by local regulations; and if cleanliness of habit, comfort of dwellings, and fresh air, be of themselves powerful elements in raising the standard of life, their influence should be felt among that class of persons. But what are the actual results? The poor workmen inhabiting the miserable streets of our large towns, and inhaling their supposed noxious vapours, are actually longer lived than the affluent and upper classes, whose easy circumstances enable them to inhabit comparatively the palaces of the kingdom.'"—Pp. 190—195.

The general result of the whole inquiry, as applied to scrofula, is given in the following manner:

"If we regard scrofula in the widest signification of the term, so as to include all diseases in which a tubercle-like deposit seems to determine the loss of life; namely, phthisis, scrofula, and tabes mesenterica, the result may be thus stated. The mortality from those diseases is larger in towns than in country districts; but it is not found that the densest portion of a town is that in which the mortality is the largest. Thus, in the four districts in London in which the crowding is the greatest, the average deaths from those affections is under 0.46; whilst in four other districts, in which there is less crowding, the average deaths amount to 0.51, and in Bethnal Green they are under 0.40.

"If we now narrow the question to the influence of a residence in towns, and bad air, in developing the mortality from scrofula alone, the result may be thus stated. In the four years' mortality, included in the fifth report of the Registrar-General, namely from 1838 to 1841, in a district comprising towns having a population of 3,759,186, the deaths from scrofula, in those four years, amounted to 758; and in a district, having a rural population of 3,440,501 souls, the deaths amounted to 1333. Or, to state the question more simply, the proportion of deaths from scrofula to 1,000,000 living, was, in the town districts, 50 per annum, and in the country districts, 97. And if with scrofula, tabes mesenterica be included, the numbers would stand as 122 deaths in towns to 160 deaths in the country."—Pp. 203—204.

On the subject of climate, humidity, and temperature many facts are given; but no specific conclusions are established.

Much has been said of late years, in public and in private, in parliament, and out of that august but rhetorical body, of the hurtful effects of factory labour in producing a sickly, feeble race of beings, in stunting the growth, and impairing the health and strength of the children there employed; and were all true, we are led to believe that the factory system would either depopulate the counties in which it was established, or so much impair the vigour and force of those employed in that species of labour, that nothing would be seen but a puny, miserable, abortive race, sickly and pining, and destined to premature death. In all this, there must be some truth; for it is natural to suppose that labour, though not very hard, yet continued through many hours of the twenty-four, in crowded, overheated apartments, and performed by children, at an early and tender age, must interfere with the development of the frame, and prevent it from attaining its full growth, or acquiring the average strength. Yet, if we examine the facts collected by Mr Phillips, throwing aside some of the general assertions advanced by his authorities, we find that there must, in these statements, be some considerable error, much inaccurate observation, and perhaps not a few erroneous conclusions.

Mr Phillips, from incontestable statistical facts, shows that the mortality among factory labourers, through the whole period of life, is little greater than the average mortality of England and Wales; that, at certain periods of life, it is positively less; and, that though, in large factory towns, as in large towns of other descriptions, the mortality is great, yet it is very little greater than in large towns without factories. In the county of Lancaster, the mortality of Liverpool, not a factory town, is greater than that of Manchester, which is the centre and seat of factories. In a series of factory towns, comprising a population of above two millions, the mortality is 2.536; while in Liverpool, without factory population, it is represented by 3.368; and London, with a smaller juvenile population, by 2.533; and, taking a set of towns without factories, embracing a population of nearly three millions, the mortality is 2.439.

When we come to specific evidence, though there is some contradiction, yet, on the whole, it is in favour of the idea, that scrofula does not prevail much in factories. Thus, we find Dr Garbutt, living in the centre of these institutions, saying that the fact is, that scrofula is almost unknown in cotton factories; and in an extensive examination which he and some other medical men made some years previously, they found that cotton factories, instead of producing, are in some sort a means of curing scrofula. In one factory examined by Dr Holme and Mr Scott, surgeon to the carabineers, of 401 persons employed, eight only were affected with scrofula; and there was no case of distortion of the spine or limbs. Dr Garbutt himself, accompanied by respectable surgeons, examined several factories, and obtained the following results:

Factory examined by Dr Holme, . .	401	8
Factories examined by Dr Garbutt, . .	504	6
. .	234	5
. .	337	4
. .	199	3
	1675	26

This is about 1.5,—that is, one and a-half per cent.

This remarkable immunity from scrofula, Dr Garbutt ascribes to the dryness and warmth of the cotton factories, to the lightness of the work, and to the superior food and clothing which the high wages of the work people enable them to obtain.

In Leeds, again, Dr Smith examined 1095 children employed in factories, and 548 children not so employed; and he found that those not employed presented marks of scrofula, or eight per cent. more than those whose days were spent within the factory houses. Mr Phillips further obtained the results of the examination of 6754 factory children, and found that marks of scrofula were observed in only 905 instances, or 13½ per cent. The returns of Messrs Faraday and Davis, and other friends, who made a comparative examination of a great many children, furnish similar results, and do not allow him to doubt, he assures us, that children employed in factories are more free from scrofula than the average of children in England and Wales.

The author concludes his inquiry with the following observations,—

"The evidence I have collected upon the question of the influence of factory labour upon human life, has produced on my own mind a strong conviction that occupation in factories, though in many respects less to be desired than occupation in the open air, is yet accompanied by so many counteracting circumstances, that the evils which may be inseparable from that occupation are mitigated if not counteracted by the increased means of procuring the necessaries of life which it affords. The evils of factory towns do not consist in factory employment, but in the agencies which are commonly found in action in such towns."—P. 235.

It appears, in short, that factory labour is by itself not hurtful to those occupied in it; that it is only when under improper regulations, and when the children are very young, or kept too long occupied, that it is hurtful; and that where sickliness or mortality attain a high figure in towns, that is owing to other adventitious circumstances connected with town life, as frequenting public houses, debauchery, and excesses of every kind, rather than the labour required in the factory.

The author enters next on the subject of treatment, and considers, first, preventative managements.

The great measures to be adopted with this view, he properly distinguishes into the choice of proper food, the administration of fresh air, and the employment of suitable exercise. For the details, we must refer to the work itself.

Under the head of curative treatment is given a history of the practice, by what has been called the touch, for which we refer the curious reader to the work, where he will find the subject, which is more curious than useful, well considered.

When he comes to consider medical touch, properly so called, he avoids the consideration of all the different remedial means and agents that have at different periods been recommended for the cure of scrofula, and confines his attention to the following series of subjects;—mercury, iodine, muriate of baryta, and muriate of lime, burnt sponge, alkalies, cod-liver oil, sea-side influences, and season, and, incidentally, emetics, purgatives, and alterative medicines.

Mercury he regards as decidedly detrimental, and therefore not to be employed at all, if possible. It is indeed well known that mercurialization very often developes the scrofulous diathesis when latent; that it always acts perniciously on those of scrofulous temperament; and that it aggravates all actually present scrofulous symptoms. He allows, nevertheless, that there are cases in which it may be given, associated with other medicines, to produce some change in the secretions, and that then it may be given. But he is opposed to its use administered so as to lower the powers, alike by profuse purgation, or by salivation.

When it is to be given, he recommends the bichloride (corrosive sublimate) as the best form; and he expresses his opinion that, in minute doses, as a twentieth of a grain, twice daily, combined with sarsaparilla, it does not yield to iodine, baryta, or any other medicines which at present enjoy a more extensive reputation.

This is not saying much for the bichloride. We add the following. We have seen it given and taken for the cure of scrofulous sores, sometimes in minute doses, as now recommended, sometimes in the form of the quack nostrum called Lignum's drops, of which the bichloride is the active basis; and though it certainly had the temporary effect of improving the appearance of the sores, and sometimes healing them, yet it did not cure the disease. The sores broke out again, and required to be healed by other means. Sometimes it seemed to do unequivocal harm, by rendering the sores more irritable, and even giving them a foul, gangrenous appearance.

Of the remedy, if we must speak ourselves, we disapprove in general terms, as most commonly hurtful. There is, however, one preparation of the medicine which we think the mildest, and which we have seen given apparently with good effect,—that is the gray powder, (*hydrargyrus cum creta*), two or three grains of which, given twice daily, is sometimes of great use as a means of improving the intestinal secretions, and thereby enabling nature to effect a cure.

Iodine he regards as not a specific, at least in combination with potass. What medicine is? Iodide of iron he regards as a good tonic, but equally void of specific powers. He thinks that, as the disease has a tendency, under favourable circumstances, to subside spontaneously, recoveries taking place while iodine was in the course of administration, have been erroneously and usually ascribed to its influence.

The day of barium and its hydrochlorate is past. It is unnecessary to revive either it or the hydrochlorate of lime.

If the presence of scrofula really depended on the operation of an acid, then the fixed alkalies, soda and potass, might be expected to be useful. They are so; but merely as means of counteracting acidity in the alimentary canal, and abating the symptoms thereof.

Burnt sponge, as containing a minute proportion of iodine, with a large quantity of animal charcoal, has enjoyed some reputation as an antistrumous agent. Since its analysis has been known, any favourable effects resulting have been ascribed sometimes to iodine, sometimes to soda, sometimes to the animal charcoal, which may have some effect as an antiseptic. Mr Phillips is disposed to deprive it of the small reputation even which it possessed; and he thinks it has never appeared to exert any therapeutic influence over scrofulous swellings. Charcoal has been said to be beneficial in various affections of the intestinal tube; and here perhaps it may produce some small influence. At the same time, its frequent failure in recent times shows that, when it seemed to succeed, the disease was subsiding of its own accord.

Cod-liver oil is one of the most recent remedies, for the antistrumous reputation of which, perhaps, we are indebted to the German and Dutch and Belgic practitioners. It is at least among these credulous gentlemen that its virtues have been most celebrated for the cure of rickets, phthisis, scrofula, and mesenteric tabes. When beneficial, the present author thinks the benefit results neither from the iodine nor bromine, which it contains in extremely small quantity, but from the animal oil improving digestion and nutrition. We should think that where it is or can be taken, the digestion must be surpassing good; and how it can be considered as a nutritious remedy, unless to an Esquimaux, it is difficult to discover. One observer remarks, that he has observed as much good to follow the daily use of fried bacon in cases of alleged scrofula; and the remedy is certainly much more palatable to the great majority of patients. Even rusty bacon would, with certain persons, be a preferable remedy to cod-liver oil.

Of sea-water, either as a bathing application or a situation for residence, he speaks rather in depreciating terms. He allows that residence there is occasionally beneficial, but the effect is temporary; and any change of air seems to be not less beneficial. Cold bathing he condemns, unless employed so as to procure reaction; when like other tonics it will produce good effects.

The conclusion, on the use of this remedy and mineral waters, is contained in the following sentence:—

> "The impression I derive from what has been stated is, that however strongly public opinion favours the opinion that sea-air, sea-water, sea-bathing, and mineral waters, exercise a direct curative influence over scrofula, no proof of this direct influence has ever been furnished. I do not deny but that scrofulous patients subjected to their influence have, during the treatment, undergone a marked improvement in the general and local symptoms of the disease; but it is probable that persons so suffering would have derived equal advantage from a judicious treatment not including either of those agents."—P. 300.

In short, we are to understand from the inquiry of Mr Phillips what has been often said before—that the only remedies for scrofula are good air and occasional change of it, good food, gentle and effectual exercise; and that no medicine except the most simple treatment, or none at all, should be employed. The following observation deserves attention :—

> "Although the materials which we possess do not enable me to offer any positive evidence on that question; and although some of the results which have been indicated, in the preceding pages, would seem, at first sight, to favour an opposite conclusion, I entertain a strong belief that the races which now occupy the more enlightened countries of Europe, have physically degenerated, and that civilization, including, as one of its agents, improved medical science, has contributed to that result. Under a less advanced civilization, the weaker and least vigorous members of a community die off, in early life, and the more robust and healthy members alone are reared. In more advanced stages of social progress, however, the case is very different. In those states, many weakly subjects are brought to maturity by intelligent care. And yet science, which has contributed to preserve the lives of the weak and ailing, can rarely give them vigorous health. When reared, they contribute to the spread of population, and not unfrequently give birth to children equally weak and ailing with themselves, if indeed their progeny do not evince even greater tendencies, to particular diseases, than the sickly parent."—P. 306, 307.

In this view there is perhaps some truth. Civilization and refinement bring their evils as their goods. The human race in many respects recede from nature as they advance in refinement, in knowledge, and in wealth. The physical degeneracy of which Mr Phillips speaks, and the maladies which are its effects and indications, may be to a certain extent diminished or alleviated; but it is only to a certain extent that this alleviation can be carried. The human race most evidently contain in their frame the principles of decay; and though a little knowledge may enable us to discover this fact, it requires less perhaps to see that we can do little to eradicate these principles, or to prevent their operation.

We have only to add, that the author has studied his subject with extreme industry; and has spared no pains to arrive at just conclusions on the causes, and formation, and treatment of the disease.

ART. II. — *Medical Notes on China.* By JOHN WILSON, M.D., F.R.S., F.S.S., Inspector of Naval Hospitals and Fleets. London, 1846. 8vo. Pp. 267.

IN all our military operations, especially when conducted on coasts and at the mouths or banks of rivers, a considerable amount of endemic disease and not unfrequently mortality to a great extent are inevitable consequences. This is the usual result wherever we are carrying on war or repelling invasion on account of our colonial establishments, and, in short, in all military operations arising out of the circumstances of these establishments. For several years past, much sickliness and mortality have prevailed among our troops stationed on the western frontier of our Indian empire and along the banks of the Indus or its tributaries; and in some seasons it has been stated that the sickliness was so comprehensive in some regiments, that the ordinary medical staff was unable to meet the exigencies of the corps.

Of all recent scenes of warfare, however, for causing sickliness, and if not mortality, at least invaliding to a great extent, certainly our expedition to China, rendered necessary by the proceedings rising out of the opium trade, has been the most eminent and remarkable. It was always known in a general way that the island of Chusan, the east coast of China, and the marshes and banks of the two large rivers flowing into the Yellow Sea, were highly miasmatic, and produced the full complement of diseases usually derived from telluric emanations—for instance, ague, dysentery, cholera, and similar maladies. The coast is in general low; and although marked in various points by rocky bluffs, there is so large a predominance of level ground irrigated in the manner practised by the Chinese, that it seems well calculated for the formation of miasmatic diseases, when it is remembered further that much of this coast lies between 30° and 40° north latitude. Then the mouths of these two large rivers and their banks, for a long space upward, are low, level, liable to inundation, and highly productive of all the disorders depending on miasmatic emanations. Indeed, the whole coast included between the two large rivers now mentioned, and the canal and sea coast, is a low flat level, intersected in every direction by streams and canals, and embanked with immense labour to prevent inundations.

The Chusan islands are situate in the Yellow Sea, opposite the outlet of the Yang-tse-Kiang, a large river flowing into the Yellow Sea in latitude 31°. 31′ N. This was the great rendezvous of the fleet. The river itself and its banks were the principal scene of all the military operations; and while the vessels had to sail up this river a considerable distance with troops, in order to carry on the necessary operations with efficiency; and while boats had to pass and repass between the vessels and the shore; and to land the troops in the month of July under a burning sun, and with the thermometer never below 80°, it will be seen that whether these circumstances were to be regarded as causes of the sickliness which followed; in the course of from six weeks to two months, that sickness was most extreme and severe; and before the close of the quarter every person engaged in active service had been ill, many twice over.

At the close of the year 1841, in consequence of the continuance of hostilities with the Chinese, uncertainty as to their termination, augmentation of force, and prevalence of disease in that employed, the Lords of the Admiralty directed a floating hospital to be fitted and dispatched with all convenient speed to the seat of war. For this purpose, the Minden, a seventy-two gun ship built of teak, strong and spacious, was put in commission, under the command of Captain Quin; and the administration of the medical department, comprising an able surgeon, five intelligent assistants, and an ample subordinate staff, was placed under the charge of Dr Wilson, the author of the present work, already well known for his "Essays on West India Fever," and his "Report on the Health of the Navy."

Of the vessel selected for this duty, all the guns excepting twenty were removed, in order that ample space for accommodating the sick, and ministering to their comforts, might be obtained. The space was calculated to accommodate 200 patients; but at certain periods subsequently it was found, partly in consequence of the great number confined to bed, partly from the heat of the climate, it was scarcely sufficient for this number.

Everything in the way of insuring ventilation, cleanliness, liberal supplies of food, medicine, and cordials, was provided on the most ample scale; in short, nothing was omitted that might be conducive to the comfort of the sick and their recovery; and we are assured on the best authority, that no such movable hospital, in respect of magnitude, efficiency, and completeness ever left any English port.

In the Minden thus equipped, Dr Wilson sailed on the 15th of March 1842, for China, and having touched at the Cape of Good Hope, Prince of Wales' Island, Singapore, and Hong Kong, after a passage occupying the space of five months, arrived at Chusan on the 15th of August of the same year. That day, with oppressive heat, the thermometer stood at 3 P.M. at 85°; there were light showers with distant thunder, and fresh breezes alternating with calms. At the anchorage, there were only the Thalia and the Wanderer, the majority of the sea and land forces being in the Yang-tse-Kiang, 300 miles north, before Nanking.

During the latter part of the voyage outward, the only complaint attracting notice was a tendency to sloughing ulcer. Any slight injury or superficial patch of inflammation was succeeded by a dark-red colour of the integuments, swelling partly œdematous, sometimes with vesicular elevations; with sanious discharge, acute pain and heat, and rapid destruction of parts. The most effectual remedy was scarification, so as to relieve the overdistended vessels of the part.

The cause was unknown, unless it could be ascribed to the combined influence of imperfect ventilation and the elevated atmospherical heat. The disease first showed itself before reaching the Cape, after the vessel had been nearly three months at sea. It ceased about the 12th September.

The diseases most prevalent on the arrival of the Minden at the island of Chusan were cholera and flux; diarrhœa appearing in the Minden and Thalia, and seven fatal cases of cholera among thirteen in the North Star, dated 26th and 29th August, at the mouth of the Woosung river. The fatal cases run their course in eight, ten, or thirteen hours.

By the 12th of September the surplus stores which the Minden brought with her were cleared, and 460 supplementary seamen and marines were discharged, so that the ship was reduced to her proper complement, and preparations for opening the hospital and receiving the sick were made. A few cases were admitted from vessels near. But the hospital was not formally opened till the 1st of October 1842.

The following account of the noted island of Chusan will communicate some idea of the position and physical characters of that locality.

> " Chusan, the chief of a large cluster of islands, is situated in the 30th degree of north latitude, and 122d degree of east longitude, distant from the nearest point of the main land of China about six miles. It is twenty miles long by ten broad, running nearly east and west, with slight inclination to north and south. Generally it is hilly, often becoming mountainous, with interspersed spaces of level land, varying in size, but rarely of much extent.
>
> " The principal anchorage, and that almost exclusively resorted to by British ships of war, is formed by a crescentic indentation in the south side of the island, and a number of isles opposite, the largest of which has been called Tea Island by the English. The harbour thus formed possesses the first requisites of such locality,—security in a high degree,—land, generally of considerable elevation, enclosing it in all directions. It is about four miles long, by two broad at the widest point, and is divided into an outer and inner harbour, by the approach of an angle of Tea Island to Chusan, and a small isle situated between them, where they approximate most closely, leaving, however, a sufficient channel for the transit of large vessels from one portion of the harbour to the other.
>
> " The rocky structure of Chusan and of the adjacent islands, so far as there has been an opportunity of examining them, is essentially argillaceous. Greywacke, and claystone, often becoming schistose as well as compact felspar, in many places assuming a porphy-

ritic appearance, are abundant. Greenstone is not uncommon, and in two or three places detached pieces of basalt have been found. Close to the sea in various situations, especially on a small island called Bell Rock, there is a coarse conglomerate, containing various large fragments of different rocks. On Tea Island there is a variety of schist, containing small disseminated portions of lime, and which appear to be the calcschist of the French.

"The highest point in Chusan does not appear to have an elevation of more than 2000 feet. The summits of the hills, as well as of the mountains, are more or less flattened and rounded in various degrees; but the declivities are often steep, occasionally precipitous; they diverge towards the sea in a central valley, between the points of which are found the alluvial levels in which rice chiefly is cultivated. These lands are naturally rich, and much pains and ingenuity are exerted by the people to increase their fertility, especially that they may obtain a more abundant supply of rice, their favourite and almost exclusive portion of farinaceous food. For that purpose, much surface water being essential to the vigorous growth of the plant, they form mounds along the beach level, to prevent the rain which falls directly, as well as that which descends from the hills, running off till it has produced its full amount of fertilizing effects. That the requisite quantity of water may be applied, shallow canals are cut at short distances across the rice-fields, from which they raise it, when under the level, by an ingenious instrument made of bamboo, on the principle of the chain pump, and by this contrivance the ground is irrigated at any time from various sources to the extent required. But during heavy rains the water confined by mounds would accumulate and lie too deeply on the surface, leaving when it was evaporated and absorbed, no provisional supply. To obviate the first evil, floodgates are formed at proper places, and canals, connected with reservoirs, and other means of supply, provide in great measure against the last.

"Besides rice, their principal cereal plants are millet, maize, and wheat, in comparatively small quantities. They have also excellent legumens; and, including sweet potatoes, carrots, a peculiar variety of turnip, onions, and bringals, diversity of nutritious potherbs, and esculent roots.

"The well-known economical agriculture of the Chinese is strikingly manifested in Chusan and the contiguous islands. Every spot of soil, from the sea beach to the highest point of land, is carefully and successfully, if not always skilfully cultivated, without regard to steepness of ascent, or difficulties encountered in manuring and labouring. On the levels a rude plough and harrow, drawn by a single ox, are employed for breaking up the soil. On the hill sides, and steep acclivities, they resort to spade, or rather to hoe, husbandry. But however imperfect the instruments employed, they do their work well. The surface is finely pulverised and thoroughly cleared of weeds, which, with the smallness of the enclosures, terraced slopes, variety of vegetable products, and diversity of colours, gives to the whole the appearance of a gigantic garden.

" As the land is continually under crop, and the same grains are sown year after year in the same places, it is evident that, notwithstanding the natural fertility of the soil, and the favouring influence of the climate, much must be done by manure, in addition to careful culture, to keep up its productive powers. They have neither lime, nor anything deserving the name of farm-yard manure, on these islands, nor do they import any. They, of course, derive no benefit from the recent discoveries respecting manures in Europe, anything in the shape of change, whether it promises improvement or not, having been heretofore altogether alien to the character of the people. In this state of things, it is curious to observe the contrivances they have adopted, independent of irrigation, for fertilizing their fields: it may also be interesting in a hygienic point of view.

" Close to the houses, which in the country are grouped into hamlets in the hollows, there are placed large earthen pans, into which are thrown all fragments of decomposable substances, to which are added the desired quantity of fluid, including urine. With heat varying from 80° to 86° in summer, destructive fermentation proceeds rapidly, and multitudes of maggots are developed, giving life and motion to the loathsome compound. Scarce anything can be conceived more disgusting than the contents of these vessels; but the Chinese disregard their offensive qualities, looking to their useful ones. They arrange them in rows along the public paths, and always place them near if not in contact with their houses, that they may at once commit to their keeping every particle of available matter. When putrefaction has proceeded to the point of giving the highest degree of fertilizing power to the mass, it is transferred to buckets, and carried to the fields, often to considerable distances, and when the ground is steep, with great expenditure of human strength; for except when yoked to the plough or harrow, man's labour is not lightened by ox, horse, or other quadruped. Placing a bamboo across the shoulders, with a bucket at each end, they carry what they consider, and what no doubt is to them, very precious stuff, in large quantities, to distant places, and often difficult of access; showing at once the value of productive soil, the industrial habits of the people, and the muscular power of the persons who perform this part of the farm labour. These receptacles of filth and sources of fertility are not confined to the country; the towns have an abundant share of them.

" At the close of autumn, after the rice crop is gathered, a portion of the soil is sown with a hardy variety of trefoil, which, notwithstanding occasional severe frosts, grows vigorously during the winter. This, which is cultivated extensively, is used almost entirely as a manure, and no doubt possesses powerfully fertilizing properties. When spring advances, and the first rice fields are prepared for the reception of seed (they being planted in succession,) the trefoil is partly ploughed down where it grows, partly carried to plots where the preparation is far advanced. Into the last, the soil being finely pulverised, saturated with moisture, and soft, with from an inch to two inches of water, on its perfectly level surface, the trefoil is trodden carefully, and with nice adjustment of quantity to the quality of the land. Under the action of solar heat, which is now becoming high, the thin stratum of water over the submerged highly succulent vegetable matter, gets speedily warm, and decomposition proceeds rapidly below, as might be expected, and as the quick succession of bubbles, and constant explosion on the surface, show.

" During winter, they store immense quantities of ice, chiefly for the purpose of preserving fish, which forms a large portion of their food in summer. The ice is taken almost exclusively from the cropped rice grounds, which, as it generally adheres to the surface of the field, has portions of stubble and earth attached. As the season advances, and the store of the preceding winter is getting exhausted by use and melting, the remainder is spread in the fields in the form of manure. That the portion of earth and straw should, as far as it goes, enrich the soil, is in the common course of things, though it would not elsewhere be thought worth the care which is given it here; but, independent of it (and they are generally knowing in such matters,) the Chinese appear to think that there is some special virtue in the water which has been frozen.

" Such are some of the husbandry practices of the Chinese, relating to manures, which are peculiar to them, and which probably influence health; and there may be others which have not been observed. It is scarcely necessary to add on this subject, that human ordure is most carefully collected, and valued to a degree which would appear not only ridiculous, but absurd, in other places.

" There are but few forest trees either in Chusan, or the islands lying near it. In some of the hollows there are a few fine camphor trees. There are also beech and elms; the tallow tree, and the cedar and cypress; acacias, and some few palmettos. On the unproductive hills, there are dwarf firs; but where the palm and pine grow, as they do here, in juxtaposition, neither can be expected to reach great perfection. Bamboo is common in most situations, and is to the people in the arts what rice is in alimentation, a substance of almost universal application. Of fruit trees, the most frequent are the pear, orange, and lemon, with a variety of cherry, but none of them so cultivated as to be finely flavoured. The sweet chesnut is also found; and they cultivate the tea plant to some extent, chiefly in the island to which it gives the name.

" Since the occupation of the island, the English have made many complaints of the water which it supplies; and some have attributed part of the sickness which prevailed two years ago to its deleterious qualities. It is stated that the ships, on some occasions, were under the necessity of taking water which had irrigated, and passed slowly over rice fields. When that was necessary, as in dry weather, with a large force, it would perhaps be, the water might prove unwholesome; it would, at any rate, soon become offensive, from the decomposition of the vegetable matter it contained. But the well water is not unpalatable, though in rainy weather it is not quite colourless, from some admixture of earthy particles, and it does not contain any mineral substance prejudicial to health. It is, in fact, properly speaking surface water, which flows from the hills, and is collected in hollow pits, having more the character of tanks than wells. But it is evident, from the extent and form of the land, that wells if sunk sufficiently, would yield abundance of spring water."—Pp. 9—14.

The capital of this island is then shortly noticed.

" Tinghae, the principal town of Chusan, is situated on the south side of the island, nearly a mile interiorly from the inner harbour, with which there is boat communication by canal. It is walled, and is said, when fully occupied, to contain nearly 30,000 persons. The wall is of irregular form, its continuous length being under four miles. When it is considered that nearly all the houses have but one story, and that there is a good deal of garden and even rice-ground within the walls, it may be thought that the alleged number of inhabitants must be exaggerated. But the streets are extremely narrow, their breadth being from eight to twelve feet, so that, with a few cases by the doors, there is often great difficulty to get a pony along, while carriages of every description are entirely unknown. When, in addition to narrow streets, it is known that the Chinese houses in towns are generally very wide and much crowded, ten or twelve persons of all ages being often in one room, the exaggeration will not be so apparent. Indeed, in the absence of clear numerical knowledge, after witnessing the mass of human beings huddled together in a small cottage, one would be disposed to allow very wide limits to the population of a Chinese town.

" Tinghae is built on a rich alluvial plain, of considerable extent, but touches, and includes part of a rocky hill in the north-east. At all other points it is bounded by the plain, which extends in an easterly direction about four miles. Over its whole extent, it is almost exactly level, and is very little above the sea at high water. A considerable stream which runs through it, is dammed at various points, to supply water to the numerous canals by which it is intersected. Here, as in similar situations, rice is cultivated, to the exclusion of most other plants; and the soil is consequently kept in the condition most favourable to its luxuriant growth. Canals are consequently carried close to the walls, and even inside of them, for irrigating the intramural fields, as well as for other purposes.

" The water in these canals moves very sluggishly, or stagnates. Their banks are lined with ooze, and abound in the animal and vegetable products of such places. Notwithstanding the value of manure, the estimation in which it is held, and the receptacles for its collection already noticed, offal is sometimes thrown into the canals, especially those of the town, which has no other drain except an ill-kept gutter in the streets, and is entirely destitute of sewers. When the rice is ripe, irrigation ceases, that the crop may be gathered, and the ground be prepared for another; and soil which had been flooded or saturated with water for weeks, becomes gradually less humid, and finally is perfectly exsiccated under a powerful sun.

"No natural marshes have been observed in the neighbourhood, nor are they likely to exist to any extent, as the industry of the people, stimulated by their wants, would speedily convert such soil into productive rice fields. But if heat, moisture, and vegetable matter excite periodic fevers, as they evidently often but not always do, few places can be supposed more prolific of their cause than the city and plain of Tinghae. Soon after the capture of the place, two years ago, the troops suffered severe loss, chiefly from fever of type and dysentery. But with the essential causes of disease just noticed, and heavy regimental and fatigue duties, insufficient supplies of fresh provisions, and irregularity or excess, which could not always be prevented, acting as accidental causes, the loss, though severe, was not perhaps surprising. Up to the date of the present year, there has been comparatively little disease, and resulting mortality. The causes of increase or diminution of force in endemic disease, are often so obscure, as to be little better than conjectural; and the essential cause is so influenced and shadowed by accidental, or, as they are called, predisposing and exciting causes, as much to augment the difficulty. No conjecture even shall be hazarded here respecting the comparative immunity of this year, so far as the miasmal, essential agency, is concerned; but in respect of the non-essential cause, whether predisposing or exciting, it appears to possess evident advantages when compared with 1840; among which the following may be specified: The soldiers have comparatively light, unexhausting duties; there is less difficulty in preventing irregularity and excess; they have abundant supplies of fresh provisions; and they are, in some degree, inured to the climate."—Pp. 17—19.

The diseases most prevalent in these localities are cholera, scrofula, affections of the eyes, often producing partial or total blindness, various diseases of the skin, as itch, dyspeptic affections, small-pox, and that peculiar hypertrophy or rather solid infiltration of the cellular tissue of the legs and scrotum called Elephantiasis or Barbadoes leg. The prevalence of these disorders is ascribed very much by Dr Wilson to the habits and modes of life pursued among the inhabitants. They are fed on coarse and innutritious food; many consume large quantities of opium and tobacco; many drink much of a weak infusion of coarse tea; all are filthy and uncleanly in their habits; and in all probability the malarial atmosphere in which they delight to live, must contribute to enfeeble the power of their frames, and render them susceptible of the attacks of disease and noxious agents.

Their habit of collecting all decomposing animal and vegetable matters, for the purpose of manure, and of allowing them to accumulate and undergo decomposition for a considerable time, must tend to contaminate the air, and unfit it to perform the part of a salubrious atmosphere. The habit of collecting all these articles, especially excrementitial products of the human race, is good in its object; but the mode in which it is done cannot fail to be prejudicial to health. It is manifest that it was never intended that articles emitting gases so offensive and noxious, were intended to be accumulated in large masses, or numerous receptacles. Were these articles conveyed away at once and applied to the surface and substance of the soil, they would not only be less offensive and less noxious, but they would be more economically employed. Much of their fertilizing properties are lost and dissipated during the process of decomposition described by Dr Wilson, and the whole of this would be given to the soil and expended on the process of vegetation, if applied more speedily and frequently.

Among other subjects Dr Wilson had the good fortune to examine the changes produced in the foot of a Chinese lady, by the practice of diminishing its size, and contracting and deranging its natural proportions. Of these changes the following account is given from the preparation.

> " In the preparation just seen the os calcis, instead of its natural broad base, and posterior rough projection, has a conical form downwards, and slopes forward from the leg, so that a straight line projected beyond the foot, from the inner aspect of the tibia, falls not before but behind the extremity of the bone. The bones of all the toes, except the great one, are bent under the metatarsal, at their points of junction, lying parallel to, and in close contact with them. The natural arch of the foot is much deepened by pressure applied to its extreme points, and the forced elevation of the tarsal and tarsal extremities of the metatarsal bones. Its depth in the present instance, measured from the inferior aspect of the bones, is fully two inches. Its depth of arch, in reference to the length of the foot, looked at as a skeleton, and without regard to its use, does not, it must be owned, give it an unsightly appearance. But the arch is not seen without dissection. It is so filled with fatty matter, that the sole is a flat surface; and the foot of a Chinese woman, in this respect, bears no resemblance to that of the Arabian, which, when helped a little by imagination perhaps, allows the brook to flow through its hollow without wetting it. It more resembles, indeed it much resembles, from its short, stumped wedge-like form, that kind of club in which the foot is generally turned inwards."—Pp. 27-28.

The hospital on board the Minden was at length opened, and on the 1st October 50 soldiers of the 98th regiment, suffering under intermittent fever, dysentery, or protracted diarrhœa, and the results of cholera, were admitted. These men arrived in the Belleisle from the Yang-tse-Kiang, where they were taken ill.

The 98th regiment had left England in December 1841, about 800 strong, in the Belleisle, a 72 gun ship, and the most of the guns having been previously removed to furnish more available space, between detachments of other corps, women, children, and the ship's company, there were on board close on 1300 persons. The voyage lasted upwards of six months, and though every means were employed to maintain the health and vigour of the crew and soldiers, Dr Wilson expresses his fear that their frames were enfeebled by the length of the voyage, crowding, atmospherical heat, the want of fresh food in their meals, and all the usual consequences of a protracted sea voyage, with two crossings of the equinoctial line. This inference it seems impossible to question, especially when we are informed that the men quartered longest on the orlop-deck, where ventilation was least efficient, suffered in a much higher ratio than others on landing.

The regiment entered the river nevertheless in July 1842, above 800 strong, and the sick-list was very moderate. Speedily, however, the miasmatic properties of the shores of the Yang-tse-Kiang began to operate on them. The soldiers disembarked and commenced fatiguing military operations under a burning sun, (90°,) with heavy baggage and arms, and on a malarial soil; and in no long time ague, remittents, dysentery, and cholera began to prevail. In the course of three months 170 men had died; 50 were sent to the Minden as already stated; only 70 rank and file were capable of performing the lightest duty; and of the rest, amounting to 510, all were ill in tents or in the Belleisle, and several were dead.

In the Belleisle herself, among a crew of 276, there were 110 on the sick-list, 12 of whom were received into the Minden on the 2d October, labouring, as the soldiers, under periodic fevers, intestinal affections, and their consequences, with ulcer. Of this number of 110, 68 were cases of periodic fever, and 30 of intestinal disease.

Other vessels continued to arrive in succession at the anchorage with different numbers of sick, mostly labouring under the same forms of disease, viz. periodic fever and intestinal disorder.

By the quarterly nosological return of the Belleisle, received on the 4th of October, it appears that during the quarter ending the 30th of September, 423 were put on the sick list, equivalent to 156 per cent. of strength; and that of these, 162 were cases of intermittent fever, 136 of intestinal flux, (13 being dysentery and 123 diarrhœa,) and 15 of constipation, making 313 of the prevalent forms of disease in China, viz.:—periodic, febrile, and mucous, independent of cholera. Of the latter, there were 17 cases, of which 7 terminated fatally. Only two other deaths occurred from dysentery and hepatic disorder, making a total mortality of 10, or 3·6 per cent. of the strength.

The Apollo, a frigate equipped at the same time and for the same purpose as the Belleisle, and having sailed with her from England, was employed in the river on the same service. Of the number placed on her sick list during the same time, which was 129, the half were cases of periodic fever and intestinal disease, exclusive of seven cases of cholera, two terminating fatally. Besides these, there was a fatal case of apoplexy, and 3 other deaths, making a mortality of six or three per cent. of the employed. The troops conveyed by the Apollo to the Yang-tse-Kiang suffered little compared with those conveyed by the Belleisle. The mortality indeed was moderate, considering the numbers attacked, and the apparent severity of the symptoms, in many cases.

In the Jupiter, employed in the same manner in the Yang-tse-Kiang, 152 were placed during the quarter on the sick list. Of these, 135 were periodic fevers and intestinal diseases, 79 of the former and 56 of the latter; the latter again being 32 diarrhœal and 24 dysenteric. Three cases of dysentery, and one case of pericarditis, terminated fatally, making a mortality of 4, or 4 per cent. of the strength.

The Modeste, employed likewise in the same river, had during the quarter a greater number of sick than any of the vessels hitherto mentioned. 249 cases were admitted for treatment; 107 were cases of intermittent, and 28 of intestinal flux, chiefly diarrhœa. Nearly the half of her complement were under treatment at the 8th of October 1842. Yet no case terminated fatally.

In the Dido, employed in the same service, during the greatest part of the quarter the proportion of sick was still higher. The number placed on the sick list was 357, when the number employed was only 200. The number of cases of ague was 204, being more than the complement, and a larger proportion than any of the ships already mentioned. The cases of intestinal disease, chiefly diarrhœal, were 38. At the 8th October, 88 cases were under treatment, of which 51 were agues. The crew were much employed in boats on various duties close to the shores of the river, which, Dr Wilson justly thinks, might occasion in her the greater prevalence of fever than in the other vessels. Five cases terminated fatally, furnishing a mortality of 2·7 per cent.

The Blonde, employed in the same service, had a very large sick list, 421 cases being entered; 246 intermittent (103 quotidian and 143 tertian), 59 cases of remittent; 30 cases of intestinal flux (26 diarrhœal and 4 dysenteric). Three cases terminated fatally = under 1 per cent.

The Childers had the largest proportion of sick; 300 on the sick list = 250 per cent. of strength. They were as follow; intermittent 124, (40 quotidian and 84 tertian), 3 remittent, 30 diarrhœal, and 70 dysenteric. One dysenteric case terminated in death.

It is unnecessary to enter further into detail. The general fact is, that the attacks of disease were very comprehensive, many persons having been attacked twice—all once probably; but that the mortality was not great.

On the 22d October, 49 men of the 98th Regiment were received into the hospital, making, with the former 50, 99 patients. Of the 49, all were suffering from intermittent fever of considerable duration, or flux separately or combined. Of the 50 received on the 1st, 27 had died and were dying; 12 rejoined the regiment advanced in convalescence, and of the remaining 11 few will ever be fit for duty. Of those last admitted many are near the end of their career; and most were in such a condition as to preclude all hope of their ever becoming efficient as soldiers.

Among the seamen and marines received into hospital, a large though not an equal proportion to that of the soldiers was admitted; and the forms of disease were the same; ague and intestinal disorder and their effects.

It is now time, however, to take a general view of the whole amount of disease affecting the troops in the Yang-tse-Kiang, during the quarter from July to October 1842. The tabular view given by Dr Wilson, and deduced from the returns of the individual ship, comes up to the 19th of November.

"The entire number entered on the medical lists, from the 30th of June till the 1st of October, was 5201, of which no more than 88 terminated fatally. The proportion dead of the attacked was, therefore, one in 59. There were 1638 cases of intermittent fever, and 1313 of intestinal flux, diarrhœal and dysenteric, independent of cholera, making 2951, or about two-fifths of the whole number treated. On the 1st of October there were 1037 remaining on the sick lists, being more than one-fourth part of the entire force. This has a formidable appearance, and is not without serious meaning in reality; for though the deaths had been comparatively few when the reports were made up, it is known that a number have happened since; and considering the insidious, too frequently intractable, nature of the prevalent forms of disease, it is nearly certain that many more must take place shortly, and that in many others, not terminating fatally within a few weeks, so much organic and constitutional injury will be inflicted, as to render the men unfit for service, the subjects of protracted bad health, and, ultimately, at no distant period, of death.

"Whatever question there may be as to the origin of so much intestinal disease here and in the north, during the late operations, there is no mystery respecting that of periodic fever. All along the flat shore of the great river Yang-tse-Kiang, as well as in all similar situations visited in China, where there are not natural inundations, the inhabitants do their utmost, and exercise much skill and industry, to convert the low lands into what may be termed artificial swamps. The profitable culture of rice, which is the principal food of the people, requires much surface water, either stagnant or moving very slowly; but the means adopted for its accumulation and distribution having been described generally in a preceding note on Chusan, and being everywhere nearly the same, as well as their effects, need not be repeated. By the means employed, they contrive to keep the level lands under, or saturated with water, during the rapid growth of the rice plant; and leave them in the same state when cultivated for other plants, through a great part of the remaining year; but whether this is with a view to profit, is not apparent. In the months of July, August, and September, the period in which vegetation, whether of cultivated or wild plants, is especially exuberant, and to which this note refers, the mean height of the thermometer may be stated at 80°, in the portion of the river occupied by the British force. Here there appears to have been abundant materials for the production of periodic fever, from which the seamen as well as the soldiers have suffered, are suffering, and must still suffer so much. The wonder is, that with such high degrees of heat acting on such a soil, they were so generally some type of well-defined intermittent; cases of remittent fever, with much precipitancy and fatal force, were not nearly so numerous as might have been expected; indeed, they were very rare.

" Of the 88 deaths, 30 were from malignant cholera. Under the general head of cholera, 107 cases were treated, 15 of which are designated 'bilious' in the reports. In five cases, the nature of the attack is doubtful; but 87 of the whole seem to have exhibited the diagnostic symptoms of the deeply-destructive form of the disease. The ships which suffered most were the Cornwallis, Belleisle, and North Star. Out of 14 ships in which cholera is noted, the first had 12 out of 40; the second, 7 out of 17; the last, 6 out of 12, fatal cases, making 25 of the 30 deaths from this disease. In some instances it had rather peculiar features; but in all the fatal ones at least, it possessed the unequivocal character and pervading aspect of the truly pestilential form of the malady called Asiatic, or spasmodic cholera.

" The proportion of cases noted as phthisical was small, no more than nine of the 5201 appearing under that head; but diagnosis, especially on board ship, in the first stage of the disease, is often difficult; and more than one case of true phthisis have terminated fatally in hospital, which were not admitted under that name. On the other hand, it is probable that all the nine cases recorded as phthisical, did not strictly deserve the appellation. But however that may have been, it may be concluded, so far as this service has given an opportunity of judging, that true tubercular phthisis is not a common disease in this particular region.

"Ulcerative cases were not proportionately so numerous as they often are in naval service; but they were, in some ships, of a bad and intractable description. In the Endymion, Dido, and Belleisle, especially, (the Minden's ship's company had also a considerable number,) they sometimes assumed a violent sloughing form, producing extensive destruction on soft parts, and involving, if not promptly arrested, the ligamentous and osseous tissues.

"The actual strength, at the beginning of July, of each ship, has not been precisely ascertained, nor her mean force for the quarter; but assuming that it was nearly equal to their complements, adding supernumeraries where they were known, and concluding that those who were known to be serving in some ships, though their number were not determined, would compensate deficiencies in other ships, it may be reckoned the 24 ships in the table had, at the beginning of the quarter, a force above—but little above—4200 men. On comparing this number with the amount of sickness, its most prevalent forms, and resulting mortality, the three following points present themselves prominently :—

First, The number sick were in the ratio of 124 per cent. of the employed; consequently, the proportion dead of the attacked was less than of the employed.

Second, Periodic fever—intermittent—was in the proportion of 39; intestinal fluxes of 31; being, together, 70 per cent. of the employed.

Third, The proportion dead of the attacked was 1·7, while of the employed it was two per cent. nearly.

The last ratio, being equal to eight per cent. per annum of strength, is not, perhaps, very formidable in itself, and seems almost insignificant, as the effect of so much disease. Still it is large, when compared with the current mortality of the navy for many years past. The annual average throughout the service, for the seven years preceding 1837, was 13·8 per thousand of the mean number employed; consequently, the loss sustained during the three months, and on the service in question, was five times as much as that usually resulting from service in the navy."—Pp. 49—52.

All the evil of this exposure, however, does not appear on this first view of the matter. Of many attacked, a considerable number have suffered so much as to render them quite unfit for active service again. The effects of ague are so lasting, and adhere so firmly to the system, and the effects of intestinal disease make so deep and strong an impression on the intestinal tissues, and through these on the system at large, that a large proportion of those attacked never recover their wonted health. This was observed long after the Walcheren expedition, the living and dying memorials of which continued to afford subject for observation for 20 or 25 years after that memorable transaction. Not dissimilar, we are informed, will be the results of the China expedition, excepting that they are likely to be blotted out at an earlier period from the map of the living. Seventy-seven, Dr Wilson states, have already been invalided and sent to England with faint hopes that change of climate might act remedially; in the certainty that for others it could do little; and that a considerable proportion would die on the passage. Upwards of 60 were taken home in the Apollo, not formally invalided, but with little expectation that they would be permanently fit for service.

The disease to which most of these evils is to be traced is what is named flux, a term comprehending, Dr Wilson states, many different forms and different appearances of intestinal discharge. When the cases were sent to hospital, the disease had been established for some time, and the period for active treatment was past. The flux was almost constantly associated with periodic fever. Diarrhœal in the commencement; it speedily assumed the aspect of dysentery; but it was also in many cases difficult to trace any distinction between the two disorders.

The following account of the aspect and physical character of the discharges we have reason to believe is extremely accurate.

> "The alvine discharges have generally been very frequent, often incessant and uncontrollable, varying greatly in different cases, and in the same case, from time to time, even in the course of a day. They have been watery simply, or watery, with threads, filaments, or lumps of mucus, with or without blood; mucous, muco-purulent, muco-sanguineous, and sanguineo-purulent; and mesenteric. Though generally very fluid, if not watery, there has been great difference in the degrees of consistence, sometimes approaching that of health, while, in every other quality, they were the very reverse; and their colour, independent of that communicated by blood, has been of every hue, from milky white to pitchy black. They have resembled gruel, melted butter, gravy soup, yeast, and boiled spinage; they had a close likeness to well-formed animal jelly, of various colours, but the most frequent being a bright bottle-green. Mixed with many kinds of morbid secretions, but most frequently with the gelatinous, there have been multitudes of small grains, like millet seeds, or uncooked sago, giving the mass much the appearance of gooseberry jam. Often there has been a substance, more or less indurated like marl, in the fluid dejections; but scybalæ were never seen. There has also been great difference in the smell of the dejections, still more difficult to describe; but it will be enough, on this subject, to say that the fœtor is generally overpowering. The above were the most constantly observed and easily described characters of the discharges by stool, but there were many others, and many modifications of them, which it would be very tedious, if possible and profitable, to delineate intelligibly."—Pp. 55, 56.

The morbid appearances found after death are given in the following extract.

"These examinations have presented in all cases, though in different degrees, extensive disorganization of parts, the seat and general character of the destructive change being remarkably alike in the whole. Ulceration has been all but universal, and obviously in most instances of long standing, the ulcers being scattered more or less thickly over the whole of the rectum and colon, of various forms and superficial dimensions, generally circular, and from the diameter of a split-pea to that of a shilling or more, the surface being purulent, hæmorrhagic, ragged, or clean and smooth, like the effect of excision; of various depths, sometimes destroying the mucous tissue only, sometimes the muscular also, being generally arrested by the peritoneal, but occasionally perforating it. In most cases, the rectum has been much thickened and indurated, in many assuming a semi-cartilaginous character; in other cases, it and the colon have been attenuated in some places while they were thickened in others; and in both, at spots free from absolute ulceration, the mucous lining has been loose and lacerable, or abraded. Frequently there has been serous infiltration of the peritoneal covering, the mucous lining having a leaden hue, through which light-coloured spots were sometimes interspersed, giving it then the appearance of dark marble; occasionally, it has had a purple colour, with a rough granular surface, resembling the exterior of a mulberry; and it has not unfrequently happened that the whole tube has been so much softened, broken down, and disorganized, as to tear on the slightest touch, or fall to pieces on being lifted. With scarce an exception, the organic lesions have been limited by the ileo-cœcal valve, the small intestines and stomach presenting a perfectly healthy appearance, although venous congestion and small spots, denoting increased vascular action, have been found occasionally. When it is said that the small intestines and stomach presented a perfectly healthy appearance, it should be added that portions of the former, generally the distal extremity of the ileum, were often, in cases of long duration, much attenuated, and pale, and diaphanous, like a prepared colourless membrane.

"In all the bodies examined, except two, the liver has been healthy, and was evidently capable of performing its functions well. It would seldom happen, indeed, in so many cases of post-mortem examination, instituted any where, without selection, that this organ will be found so thoroughly entire. The exceptions alluded to were, first, one in which there were tuberculous deposits, which however had no apparent connexion with the disease which caused death; and, second, the case of abscess noticed in a former paragraph. There was one case of abscess in the spleen, also noticed in a preceding page, and another, in which there was a small encysted formation of pus; but the latter, at least, appeared to have had little if any connexion with the fatal issue; and in a vast majority of instances, indeed, all but universally, excepting the immediate seat of the disease, the extent of the large intestines, all the organs and tissues of the body were in a remarkably sound condition."—Pp. 56—58.

Though Dr Wilson has no doubt, that on the banks of the Yang-tse-Kiang and the coast of China, intestinal disease is the result of miasmatic influence, and is the allied or kindred offspring with ague of that influence, yet he allows that there are other situations, in which it is not only difficult to prove this, but reason to believe that the intestinal disorder may not be dependent on miasmatic influence. Thus he observes dysentery is much more frequent at the Isle of France than on the west coast of Africa, though in the former locality there is little marshy land, while in the latter it is abundant; and dysentery is much more prevalent at Bermuda than Belize, though the former place is almost void of marsh, while at the latter this is almost the only sort of soil. In the Chinese river, and along its banks, the two are so closely connected, that the intestinal flux may be regarded as part of the ague; and it is indeed often so prominent as to disguise the original affection, and divert from it the attention of the observer.

Even the ulcerative form of disease already noticed, that is, the occurrence of bad sloughing ulcers on the surface, Dr Wilson is disposed to ascribe to the same cause. The effect of this was, as shall appear afterwards, not only to induce in the neighbouring parts a peculiar destructive or phagedenic action, but to render the patient pale, feeble, anæmial, with white wax lips, and in short to give him all the appearance of a person labouring under the agency of a morbid poison.

The telluric or marsh and soil poison, indeed, along the banks of this river appears to be of the deleterious character, and peculiarly hurtful to the European frame. It seems to be equally powerful, severe, and rapid in its effects as that of the African rivers ; and greatly more so than in any of the other Asiatic rivers. Nor was its operation always confined to specific, palpable forms of disease. Sometimes without producing direct open ague, or dysentery of a manifest or violent character, it acted by generating a peculiar state of debility of all the textures and organs, as if it had poisoned the whole mass of blood, and caused that fluid to act on the textures as a poison, instead of the vehicle of their nutrition. Thus, not only did it cause sloughing ulceration of the integuments, as already stated, and rapid destruction of the tendons and ligaments when affecting the foot, denudation of the bones of the leg and foot, and death from the severity of the whole process, but it caused gangrene of the fingers and hands, with fever so violent as to cause separation, and narrowly allowed the patient to escape with his life (p. 81.) Another effect which Dr Wilson thinks may be traced to it, is that of general debility in the whole frame, languor of all the functions, without any indication of local lesion sufficient to account for the prostration of the powers observed. Of this he gives an instance, of which we subjoin a short abstract.

Lieutenant R. Ancill, agent of transports, aged 50, had served long in various parts of the world, and had suffered severely from intermittent fever. In the course of duty in the Yang-tse-Kiang, he had suffered severely for many weeks from some form of periodic fever, which nevertheless did not recur distinctly for more than one month. Subsequently, he had suffered from flux, fainting fits on attempting to rise, and afterwards by rapidly increasing debility. On the 12th December, he was received into the Minden, and, though slight symptoms of rallying ensued, they were temporary and inefficient. He was pale, sallow on the surface, slightly dropsical; the gums were flabby and pale, the breath fetid, the tongue white, the pulse at 104, and feeble. The patient continued prostrate with torpor, and tendency to somnolence, listless, and languid, giving answers not indicative of his state. The power of receiving food was nearly gone; and there were no proofs of any remaining power of assimilation. He died on the 24th.

Inspection of the body 30 hours after death disclosed no lesions proportional to the symptoms during life. A small quantity of serous fluid within the cavities of the *pleuræ* and arachnoid membrane; dark-coloured patches on the mucous membrane and the intestines; and the spleen a little soft and lacerable, were all the changes that could be recognised. The brain, heart, lungs, liver, pancreas, and kidneys, all were healthy.

This must be regarded as one of those cases, not perhaps frequent, yet not very rare, in which the miasmatic poison, being mixed with the blood, meets, however, with textures not firm enough and vigorous enough to produce energetic reaction. It operates, then, in a slow, gradual, and insensible manner, decomposing the blood, and rendering it unfit to nourish the organs, or to produce the healthy, natural stimulus to the textures; and hence, from the twofold circumstance of the decomposition of the blood, especially its red globules and its fibrin, and the feebleness of all the textures, proceed probably the sallow colour of the surface, the dropsical tendency, and the general debility. It is not easy to say where this tendency to decomposition from the paludal poison begins. Probably it is first in the lungs, through which it must pass to get into the blood.

About the end of 1842, and the commencement of 1843, chiefly from the circumstance of the lower temperature, the thermometer ranging from 43° to 52°, some improvement in the health of the forces took place. Agues had become rare, and were mostly the remains of former attacks. Yet these were not extinct; and ulcers and flux continued to appear and commit ravages.

The Apollo, already mentioned, was to proceed to England, and sailed on the 21st January 1843; and her history suggests several instructive observations on the comparative salubrity of different vessels, and the causes of the difference. She had left Plymouth with troops in December 1841, in company with the Belleisle and Sapphire; proceeded to the Yang-tse-Kiang in July 1842, and remained there until the signature of the treaty of Nanking; and was afterwards employed upwards of three months at Chusan. During the whole period, she continued remarkably healthy. Into the cause of this, Dr Wilson was led to inquire; and the only peculiarities to which he can ascribe this comparative immunity from disease, were the following. Water was admitted into the lower part of the vessel, and pumped out regularly and frequently, but with variable frequency, according to the state of the weather. When the weather was hot, the operation was performed daily; when the temperature was not very high, it was done three or four times a-week. By this contrivance, though the space under the limber boards was never absolutely dry, Dr Wilson conceives that, along with slight leakage, various impurities were incessantly conveyed away, and were not permitted to accumulate.

This fact leads him to recur to the views which, several years ago, in his Essays on West India Fever, he laid before the profession. It must be known and remembered by those conversant with these essays, that Dr Wilson saw reason to infer, that the main cause of unhealthiness in ships in tropical countries was the drying process going on and carried to an extreme degree, in the timbers of the vessel, and that vessels were unhealthy usually in proportion to the extent to which this process was carried. It was demonstrated by Dr Fergusson, that it was chiefly the drying process in the marsh that at a certain stage became productive of endemic fever. Dr Wilson in like manner sees reason to think that the drying process, in similar situations and circumstances, must be productive of the same result; and, as the timbers of vessels undergo a drying process in tropical climates soon after their arrival, and indeed are not unfrequently intentionally exposed to the process, and afterwards become remarkably febriferous, he infers, that, if by any means this process could be prevented, or at most, not allowed to be carried to the febriferous point, then the vessels would be comparatively healthy and free from fever.

"For similar reasons," he says, "and by an analogous operation, it may be, that the frequent admission and expulsion of water into and from a ship, may prevent or remove the emanations injurious to health, generated within her. It may be that there is strong affinity between the emanations in question, and water thus admitted, and that thus the former are rapidly and effectually discharged with the latter, nearly as fast as they are formed, and consequently without effecting much mischief. The operation must have the power of ventilating, to a certain extent, a part of the ship where ventilation is a very difficult, almost an impracticable process; and thus, by mere mechanical action, prove beneficial. But by whatever means, and to whatever extent, the practice may prove, or have proved, effective in the preservation of health on board ship, it is so simple and easy of application, that it should not be allowed to pass without consideration, and ought to be fairly-tested.

"Its consideration naturally leads the mind to the essential cause and prevention of fevers originating in ships, especially of that which occurs so frequently, and proves so fatal, in the West Indies. The writer served some years in that part of the world, saw much of the disease, was interested in its operation, and studied it with some care; but having laid the result of his observations before the profession, he will not, as indeed he ought not, except incidentally, enter on the matter in this place. He will limit himself, at present, to saying, that he satisfied himself, when on the spot, and no subsequent information has shaken the belief, that the cause of the disease is often in the ship, and deduced from her, being an aerial product of her decomposable materials, though associated with, and in some way dependent on, a morbific atmosphere, emanating from adjacent land; that such is most evidently its source, when it becomes very prevalent, and possesses much fatal force, as often happens at sea; and that it is generated independently of interior want of cleanliness, accumulation of vegetables, or other filthy and rapidly decomposing substances, on or under the decks; as ships which are absolutely clean, in the strictest sense, in every part which can be reached, not only on deck, but in the holds, store-rooms, &c., suffer often most severely."—Pp. 90—91.

"In accordance with the view that dryness, at the lowest points, is not only desirable, but obtainable, to the degree, at least, in combination with perfect cleanness, of favouring health, it was common sixteen years ago, and the practice was continued not long since, to institute, soon after arriving in the West Indies, especially if there were any appearance of fever becoming general in a ship, a thorough process of clearing, cleaning, and drying her in every part. The means usually employed were, taking all stores out, and exposing the whole interior, from the hatches to the keel, whitewashing, and letting down stoves for some time, till all doubt of dirtiness or dampness was removed. But it happened frequently—more frequently than otherwise—that fever, if it had begun, instead of being arrested or moderated, became more frequent and severe after than it had been before; and often set in at no distant period, if it had not shown itself previously. This relationship the writer pointed out, and offered some remarks on, both in the 'Memoirs of West Indian Fevers,' and the 'Statistical Report on the Health of the Navy,' in the West Indies, especially the former. He was satisfied then, as he still is, that the process in question did not answer the laudable end for which it was ordered, that, namely, of staying or retarding the march of the destructive disease; nay, that it often precipitated the eruption of the disease, and rendered it more violent."—Pp. 91, 92.

He is accordingly led to recommend the frequent introduction of water, and its subsequent expulsion, in order to remove all gaseous and other hurtful products, as a means of preserving a vessel in a state of health.

"Looking at these things together, the obscurity still surrounding the origin of ship fever, more particularly the West Indian, and the conflicting opinions to which it has given rise, it becomes a pressing question, more especially as applied to that region, whether the attempts to keep the more dependent parts of a ship dry should be continued; or whether it would not be advisable to abandon, at least for a time, attempts which are not successful; to limit, but enforce, dryness on the decks where the crew live, and other places where it can be perfectly obtained; and to wash out frequently and thoroughly the other parts, by the free admission and forcible expulsion of water. At any rate, this method of purifying the inferior parts of vessels, which is recommended to consideration for the service generally, is eminently entitled to a fair trial in the West Indies, since the practice heretofore generally adopted has failed, too often signally; and as experiment alone can decide the question, experiment should not be spared. There is a large mass of evidence tending to prove that the cause of West Indian fever on board ship when it becomes epidemic, and endued with much fatal force, is essentially dependent on a chemical process, proceeding within the vessel, and that it proceeds principally from her lowest interior parts, to which the ordinary means of cleaning, drying, and ventilating, cannot effectively reach. It might be, as was suggested above, that if fresh water were admitted daily, the febrific emanations would be absorbed, soon after they were set free, and discharged speedily and effectually by the water, when ejected. It is certain that the spaces under the limber boards and lining would be kept more thoroughly clean in this way than they generally are; and thus, apart from all problematical, though important advantages, a palpable benefit would be conferred."—Pp. 92, 93.

He is then led to advert to the case of the Eclair, as an instance of fever being produced within the timbers of the vessel. On this subject we have already spoken in our 65th volume, and we then gave the opinion that this was the source of the disease, against which all the quarantine restrictions were so thoughtlessly and foolishly enforced; and we are happy to observe that Dr Wilson takes the same view.

"Everything," says Dr Wilson, "in the progress, character, and cessation of the disease in the Eclair, so far as the writer has the means of judging, points to its local origin, and shows that it was generated on board, its cause being the product of chemical action within the vessel, probably in her lower and least accessible parts."—P. 93.

In truth, there never was any thing like contagion in the disease of the Eclair; and whatever a set of unreflecting, inconsiderate, official persons may say or do, not only were these precautions useless, but by the delay to which they led, and the long time the sick were confined in the vessel, they did infinitely more harm than the most complete neglect could have caused. So foolish and unreasonable a being is man when he is guided by ignorance, prejudice, presumption, and confidence.

It is impossible to form any other opinion of the quarantine laws and restrictions, than that they are not only the offspring of the most gross ignorance and incapacity for observation and reasoning, but that they are kept in force for the advantage of a few indolent, unthinking persons, who find it much more easy to maintain old prejudices than to look at the truth, and to maintain a system by which they are entitled to receive handsome remuneration for doing a great deal of mischief, than to confess that this system is founded in error, and has been, and continues to be, the cause of much evil. The restrictions on commerce are sufficiently hurtful and sufficiently absurd. But the worst objection against them is, that they cause, without any advantage, much waste of time, and consequent pecuniary loss. But of the other parts of the system, enjoining the separation and detention of so many human beings, the effect is too often to aggravate the evils which it presumptuously pretends to prevent, to induce disease where it did not exist, and certainly to increase mortality. If the timbers in a ship be actually a source of disease, can there be anything more absurd, or more cruel, than to insist on confining the crew within the vessel until they either, by the blessing of Providence, recover, or, in the ordinary course of events, die? What a heavy responsibility is incurred by those men who, with infection, contagion, and importation, constantly on their lips, and with the most gross intellectual blindness, overlook the true causes of the disease, and have recourse to measures useless and pernicious, and which, were they not supported by artificial institutions and national laws, must make them guilty of homicide on the great scale!

Of evidence, and decisive evidence, on this subject, there is no want; neither is it one on which information is scanty or deficient. But the painful part of the whole business is this, that a set of persons who ought to be informed, who ought to judge from facts, and whose minds ought to be clear from prejudice, will not take the trouble to study these facts, adhere to their old preconceived notions, and form their decision with as much certainty, as if it were impossible for so weak, so feeble, so fallible a being as man ever to be in the wrong. These persons we recommend to study, not only the present observations in the work of Dr Wilson, but his remarks and illustrations in his former work, which contains evidence that ought to satisfy any candid inquirer into truth.

Dr Wilson remained till the beginning of May at Chusan; and on the 8th of that month he sailed in the Minden for Amoy, on the passage to Hong-Kong. The author, before quitting the place, is naturally led to bestow a few thoughts on the causes of sickliness and insalubrity so manifest and prevalent at Chusan. This he thinks is not owing to the natural or innate character of the country, but entirely to the ignorance and perversity of the human race. He thinks that it is mostly, if not entirely, "the wilful work of man's hands, or of his neglect and perverse ignorance." The main cause of the sickliness of the China coast in general, and the island of Chusan in particular, is, Dr Wilson thinks, the system of irrigation pursued for the growth and culture of rice; and he, therefore, infers that the proper and effectual remedy is, to substitute for the cultivation of rice that of wheat, which, he argues, would at once clear the country of nearly every quality allied to those of marsh land, and would render it infinitely more salubrious, if not remove almost all sickliness entirely.

Next to this great revolution, for a great revolution it undoubtedly must be with the Chinese, are such measures as would correct the more limited morbific influence created and fostered by man, as the results from canals within and around towns, and the conditions of towns as to form, structure, and sanatory municipal regulations. The canals would require, he maintains, either to be cleaned out frequently, or filled up entirely, which would be preferable; and as the cultivation of wheat would not require them, this might be practicable. Towns would require to be built with wide and spacious streets; gutters, sewers, and drains would require to be constructed; and means would require to be adopted to ensure the speedy and frequent removal of dirty rapidly decomposing matter from narrow crowded lanes. When all is the reverse of this, under a temperature ranging from 80° to 88° and 90°, it is less surprising that the coast and towns are unhealthy, than that they are not perpetually pestilential.

On the 14th of May the Minden arrived at Amoy with a fine southerly breeze and bright sky; the thermometer at 80°, the barometer at 29° 85′.

Of this place the following account is given.

"The island of Amoy, separated from the mainland of the Fokien province by a narrow channel, is situated in latitude 24·30 N., longitude 118 E. It has an irregularly winding outline, with a deep indentation of the sea running from west to east, and dividing it into two nearly equally-sized peninsulas. The longest diameters are from east to west, and from south to north nearly, being about seven miles each, including the estuary alluded to above. At least nine-tenths of the entire surface consists of a ridge of granite, precipitous and rugged, and barren as granite of such a description must be. The highest point is estimated at 800 feet above the level of the sea, but the elevation differs much at different places, and there are some deep chasms. The whole of the ridgy surface, constituting so large a portion of the island, is incapable of producing food for man or beast, and supports but in few places small quantities of stunted shrubs, which are not seen at any distance; and thus the impression of absolute sterility is, at first sight, communicated. The impression, indeed, is nearly correct. Little can be forced from the soil here, and the large population depends chiefly on the opposite fertile island of Formosa for the necessary supplies of food. But wherever there is a level bit of land, or land that can be levelled, however small or naturally unproductive, the agricultural industry of the people makes the most of it; and by diligent culture, manuring, and abundant application of water, they procure astonishing crops of rice; by the two former abundant returns of sweet potatoes. These and potherbs are their principal products. One of the largest spaces of level land, and producing the greatest quantity of rice, is on the west side of the island, close to the beach, near the anchorage, and not far from the city. Another of considerable size is within the city, and nearly in its centre.

"Opposite to the west side of Amoy, at the distance of about a quarter of a mile, at the narrowest point, is the Isle of Kulungsu, extending about a mile from south-east to north-west, and half a mile from east to west where it is broadest. Such being its limits, its extent is small. Like Amoy, it is a naked granite rock, with less elevation, but containing perhaps even a smaller proportion of cultivable surface. On the south shore, near the barrack of the 18th regiment, there are a few patches under rice, sweet potato, and indigo cultivation; and there is a similar larger space to the westward of the barracks; but the greater portion of it bids complete defiance to even the persevering labours, and alimentary exigencies of the Chinese. Close to the beach, south-east, a short distance from the barracks, there is a level of some extent, perhaps five or six acres, lying between two ridges, on one of which there is a fort, and an hospital is erecting on the other. This level, which is directly to windward of the barracks, was formerly a rice field, but is now neglected, and from stagnant water, with the spontaneous growth and decomposition of plants, has degenerated into a swamp in the strictest sense of the word. Within the area of the barrack buildings there is another small swampy spot, which however is in progress of being reclaimed.

"Barren, and impracticable for productive purposes, however, as

Kulungsu generally is, it is not destitute of interest, and even beauty of a peculiar kind. Previous to its capture and conditional occupation by the English, it was thickly sprinkled with houses and cottages of a superior sort, capable, it is said, of containing 3000 persons, and forming a kind of Highgate or Richmond to the opposite city of Amoy, to which the citizens resorted for the purpose of recreation, and that they might breathe a purer air, and look on more pleasing objects than the confined crowded unclean streets of the commercial town afforded. The change must have been charming as well as beneficial, for dwellings did not so press on each other as to shut out pure air and the sight of natural objects. They were always in pleasing situations, often in spots of romantic natural beauty; under cliffs, among huge fragments of rock, or in small dells, often graced by flowering shrubs, sometimes overshadowed by wide spreading trees. They are now, except those occupied by troops, untenanted, unroofed, battered by shot, and falling to the ground."—Pp. 117—119.

" The harbour of Amoy is situated between that island and Kulungsu, but there is good anchorage to the south of the latter, on the west side of the former, sufficient security, within large space, being afforded to ships by Amoy on the east, numerous small islands on the south, Kulungsu and Pagoda island on the north and north-west, and the mainland on the west and south-west aspects at the distance of about seven miles, forming together an almost continuous chain of protection, and a capacious harbour for ships of any size.

" Here, generally on the east shore, and partly close to the west side of Amoy, and south point of Kulungsu, the large and some of the small English ships of war anchor, besides many of the native, and some foreign vessels employed in trade.

" The city of Amoy stands, as has been stated, in the most westerly point of the island, opposite to Kulungsu; and considering the smallness and sterility of the island, and the unproductive nature of the contiguous mainland, contains a prodigious mass of inhabitants. The population is computed variously by different persons possessing knowledge of the subject; one account, which is the highest, rating it at 300,000. On asking a principal Mandareen, he gave it at 40,000; but the interpreter alleged that he had included in his reckoning only male adults, which, when the large proportion of children and women is remembered, would bring it near the highest estimate. The Mandareen's account of the matter, however, judging from other relations in which the writer had better means of testing his knowledge, was not worth much. Here indeed, as elsewhere, the ignorance amounting to utter childishness, of the people, whatever their class, in all things not relating directly to the means of sustenance or sensuality, is amazing. The population, however, is very large evidently, judging simply by the size of the place, and knowing the immense number squeezed into a given space in Chinese cities, as their narrow streets, small tenements, and crowds inhabiting them, show.

" The original walled part of the city occupies the summit of a hill, some 200 feet above the sea; but by far the greater portion of it, perhaps eleven-twelfths, is extra-mural, and extends along adjacent levels, or up on the lower acclivities of the hill. After what has been noted respecting Tinghae, it would be tedious and irksome to give a detailed account of the form, structure, and municipal economy of Amoy. In these respects, the description of the first may be applied almost literally to the last. Multiply Tinghae by ten, and you have a distinct general idea of Amoy. There are here the same narrow streets, from eight to twelve feet wide, as there; the same wretched pavements with holes instead of gutters, or where there are any thing like open continuous drains, their contents are often stagnant, putrid, and fœtid, as they are never cleaned by the hand of man, but left to the cleansing power of rain or other natural causes; and there is total want of provision for ventilating the dwellings. These attributes of a Chinese town are as inseparable from it as the stones and tiles of which it is composed. Hygieia has no place among their many deities; and of any thing pertaining to her worship they have neither knowledge nor practice. Here, however, the streets are not only much longer, but more irregularly disposed, being more bent and twisted, rendering them more obstructive to the circulation of air than at Tinghae; and there are some other differences between the two places which deserve notice.

" Here, especially in the narrowest streets, pent houses are placed in parallel lines, from each side, near the centre, so as to leave only a few inches between them for the descent of rain and passage of air. When the extent of this systematic opposition to atmospheric change is considered in connexion with the accumulated population; with the want of the commonest means of ventilating houses, such as opposite apertures in the walls of buildings, having three or four apartments running into each other laterally, and back from the streets; with their earthen floors, and the absence of cellars, sewers, or other channels of underground purification; with stagnant gutters and pits of putrefaction in all directions; and with atmospheric heat, varying between 80° and 90° of Fahrenheit's scale for five months in the year; it is astonishing that the place is not swept by fearful epidemics and ere long depopulated. Amoy has a species of street appendage, from which Tinghae, for the time at least, is free, namely large open jars of urine, which occupy conspicuous places along the walls at short intervals. But Amoy is not surrounded by oozy weed-covered canals like Tinghae, nor is the town much intersected by them, because the form of the ground does not permit their continuous formation; and because there is comparatively little soil that can be turned to rice cultivation. Thus the inhabitants are saved, to a considerable extent, from one source of aërial contamination, affecting those of Chusan, not willingly indeed, for, as has been observed, wherever land can be got to bear rice, it is eagerly employed for that purpose."—Pp. 119—122.

The diseases are the same as those noted at Chusan. Cutaneous affections, including leprosy of the most loathsome kind, and diseases of the eye are common. Intermittent fevers and dyspeptic disorders also prevail, and the common intestinal disorders and cholera occur at their seasons.

The predominant forms of disease among the troops were the same as in the Yang-tse-Kiang, that is, flux, fever, and ulcer, with the difference of a large amount of remittent fever. Remittents are indeed more frequent, and intermittents more rare than at Chusan and on the banks of the Yang-tse-Kiang. This the author ascribes to the higher temperature, the thermometer ranging in the outer harbour of Amoy, from April to October, between 80° and 90°, which is a little higher than at the former stations. The loss sustained by the troops here was very inconsiderable.

In the hot season of 1844, however, cholera was very fatal at Amoy and Kulungsu.

From Amoy the Minden proceeded to Hong Kong, where Dr Wilson arrived on the 7th June 1843. The vessels there stationed, viz. the Cornwallis, Thalia, Agincourt, Childers, two steam vessels, two surveying vessels, and a troop ship, had all large sick-lists, but few formidable cases.

The prevalent diseases were periodic fever and intestinal disorders; sloughing ulcer being almost unobserved; and such ulcers as were observed in the Agincourt being supposed to have a limited origin, and to be unconnected with atmospheric agency.

Remittent fever was more prevalent than at the other stations; and sometimes it assumed a malignant character, affecting the sensorium so violently as to be called by the men head-fever. In some few instances it destroyed the persons attacked in the invasive or congestive stage; and then the leading symptoms were vertigo, producing paralytic tottering gait, or preventing motion entirely, with or without violent headach; drunken expression of countenance and eye; dilated pupils; indifference to external objects; rapid, feeble, fluttering, often intermitting pulse; the surface drenched in sweat; the skin cold, and almost lifeless at the extremities; vomiting and purging of bilious matter; jactitation; thirst; white soft tongue; lividity of the lips, with dusky complexion, and the same colour extending to the rest of the body. Remedial measures in such cases were in general unavailing and ineffectual.

When the attack did not terminate fatally, it shewed itself either as remittent, ending in ague, or proceeded to ague, with intestinal disorder.

" In one respect there is much resemblance between this fever and the proper endemic of the West Indies, namely, the apparent want of consent between the real force of the affection and its external, appreciable manifestation. Here, as there, when there is no complaint made or admitted, and while there is no violence in the febrile signs, it not unfrequently happens that the subject is suddenly seized with hurried respiration, fluttering pulse, partial unconsciousness, or slight coma—a condition which speedily terminates in death. But while there is this point of agreement, there are many points of difference between the two forms of fever. It would be tedious to go over the whole, which, taken together, form a distinct line of diagnosis; but one may be noted in passing. In West Indian fever, death generally ensues, or convalescence is fairly established in a few days; for the most part, within a week; and when established, it proceeds steadily in most cases, speedily to completion. Relapse is remarkably rare; and the subject, in a short time, is as well, strong, and little liable to disease, as he was before the attack. In the Hong-Kong fever, on the contrary, when it is subdued, its duration being very uncertain, intermittent fever, or flux, as already stated, constantly sets in. There is great proneness to relapse; and, after the disease has been apparently subdued, and the tendency to recurrence overcome, the subject continues long listless and emaciated, has sallow countenance, with pale lips, and hovers on the verge of jaundice, dropsy, or fatal flux. In enumerating the most prominent symptoms of the recent cases, it was stated that the patient sometimes sunk before the completion of one period of the fever, a statement equivalent to saying that death occurred within twenty-four hours; and this in strong muscular men, who had been in perfect health a few hours before. It seldom happens that disease assumes a more formidable aspect than this."—Pp. 133—134.

The information afforded by morbid anatomy threw little light on the nature or cause of the symptoms.

"In the worst, suddenly fatal cases, nothing was detected, excepting a loaded state of the large venous trunks, and a certain degree of venous congestion of dependent parts in the thorax and abdomen. The brain and its investing membranes were more exsanguined, and consequently paler than in normal condition, there being so serous effusion either under the meninges, or into the ventricles. Neither here, nor in other parts, was there anything which, with the utmost latitude of language, would be considered inflammatory, or having the least approach to it. The vascular condition of tissues was, indeed, the very reverse of that which constitutes inflammation.

"When the disease was more protracted, became distinctly paroxysmal, and did not terminate in death for some days—from five to fifteen—the organic appearances observed on dissection had a similar pervading character, but were, to a certain extent, modified, with additions. What could be ascertained of abnormal in structure, was in these, as in the former, with little exception, congestive, not inflammatory, especially manifested in the lungs. In two cases, there were inflammatory patches at the great curvature of the stomach, and in two others, similar patches on the dura mater—conditions which, there is reason to think, from the previous symptoms, arose shortly before death. In three instances, of considerable duration, the cortical portion of the brain had a peculiar, uniform, dark-colour, approaching that of worn mahogany; but there was neither abnormal softening, nor induration, that could be traced; nor was there more, there was perhaps less, than the usual quantity of effused fluid. When fever had followed flux, which was not uncommon, ulceration was occasionally found in the colon, the result evidently of the preceding affection. The integrity of organs, including the liver and spleen, when the violence of paroxysms in many cases is considered, is surprising."—Pp. 134—135.

The following observations deserve the attention of those who are anxious to localize fever, and ascribe its symptoms to lesion in one or other individual organ or apparatus.

"Neither the morbid phenomena, nor the post-mortem appearances, in these cases, gave any support to the theory of localism, or the doctrine which teaches that there is no such disease as essential fever; that universally it is a symptomatic, not idiopathic affection; and that, however difficult to distinguish, and however frequently therefore overlooked it may be, inflammation of an organ, chiefly of some part of the encephalon, or alimentary apparatus, is in every instance present; and that, not as a part, concomitant, or result, but as constituting the essence of the disease, and the cause of all consequent symptoms. The preceding sketch of appearances, during the disease, and after death, leads to another conclusion. It shows that the fever was primarily and emphatically a disease of the whole system, though it might not affect every part of it equally at the same instant. Its power was especially manifested in the sensorial and vascular tissues, through which secretion was perverted or suspended, excretion was arrested or, rendered inordinate, and motive power was paralysed. The pestilential miasm acted, in many respects, like a concentrated narcotic poison. It struck suddenly at the source of vitality, if so vague an expression of an obscure condition may be employed, producing so much prostration of the organic force, and consequently of their necessary operations, as, in some cases, to destroy life speedily; and, as has been remarked, when the fatal course was very speedy, and there was no true reaction, the process by which life was extinguished left scarce a trace behind it." —Pp. 135—136.

Hong-Kong is in short pre-eminently an unhealthy situation. In the end of September, and far on in October, disease, chiefly periodic fever and flux, continued its ravages; and one or other, often both, of these diseases seem to have made so violent and deleterious an impression on the systems of the patients, that the medical attendants had little to do but to deplore the inefficacy of their art. Though several, indeed many of the cases at first recovered, yet afterwards they relapsed and terminated in flux, with manifest indications of great and irreparable destruction in the intestinal tissues. This was the state of matters at the close of September; and the inefficacy of medical treatment made the medical staff look anxiously for change of weather and temperature to aid their efforts.

"The diminutive colony of Hong-Kong, where so many hopes and expectations are centred, is situated at the mouth of the great estuary of Canton, eighty miles below the city, close to the left bank, being separated from the continent, at one one point, by less than half a mile. Its geographical position is in 22 N. latitude, 114 E. longitude. Its largest diameter, which runs nearly east and west, is nine miles; the breadth, from south to north, is five and a-half miles. In shape it is very irregular, having numerous bays, and some deep indentations, with long projecting peninsular points, which render the space comprehended by the above short lines less than the measurements indicate; and from the precipitous form of the hills, independent of inherent sterility, it affords little scope for agricultural industry. It is one of a multitude of islands by which the coast of China is guarded, and which, among other natural advantages, raise her above any other equal division of the world, in the number, extent, and security of her harbours; and that, or rather those, of Hong-Kong, are inferior to none of them.

"The principal is on the north side of the island, facing the rising capital of Victoria. Here, without tracing it east to Tamtoo, or west to Lantao, which would give a length of fourteen miles, there is a compact haven, about three miles square, which cannot be surpassed in the qualities which constitute a perfect anchorage. It is formed by Hong-Kong to the south, by that and the mainland, the former bending on the latter, to the east, by the continent on the north, and by the island of Lantao to the west. By these it is not only, in nautical phrase, land-locked, but strongly guarded by the height of the surrounding land, which rises to 3000, and scarcely falls below 1000 feet; so that, when it blows strongly outside, there is little disturbance of its surface. It is easy of access by an eastern and western passage, but the latter, being the more capacious, is most frequented. These opposite openings contribute to the ventilation and cooling of the bay and contiguous town, which otherwise, during the southerly monsoon, would be much, and by so much, more intolerably hot than they are.

"Considered geologically, Hong-Kong may be said, in general terms, to consist of a mass of granite, as there are few and inconsiderable portions of other rocks, either incumbent, or, as far as can be observed, interspersed; and the amount of resulting soil is remarkably small. Gneis, quartz rock, sienite, sandstone, and compact felspar, occur in some of the lower acclivities and levels; porphyry and basalt are said to exist, but they have not been seen by the writer. The granitic formation has a twisted ridge form, from east to west; but the line is broken by deep chasms and rising points of many shapes. Towards the base, the granite is, in many places, in a state of disintegration, more or less complete, but is mostly of a firm and enduring structure, as its crested points, precipitous faces, and want of soil, testify. When not nearly perpendicular, as it is in many places, it is generally in lateral ridges, running at right angles nearly from the great central ridge, and terminating in spurs near the sea, which, as they recede from each other, leave little valleys, bounded by the beach, and forming the sole small cultivable portions of the island. The geological composition of the continent opposite, as far as it has been observed, is similar to that of the island.

"Victoria, the seat of government and of commerce, stands close to the base of nearly the highest, if not the highest point of the ridge, on the north side; and so close is the base of the mountain to the beach, that it is necessary to make a long winding street in the direction of its sinuosities, and to erect the greater portion of the buildings on heights and practicable slopes. Such slopes, however, do not extend far from the sea. At a short distance, the rock rises abruptly, at some points almost vertically, to an elevation of near 1800 feet. This proximity of base to the beach is common to the island, the small cultivable portions of surface being embraced by the receding mountain spurs noticed above. The soil in these narrow vallies and ravines, and on the lower slopes, is a dense ferruginous clay, naturally so sterile, notwithstanding the favouring influence of heat and moisture, as to produce nothing but stunted firs and palmettos, dwarf laurels, cactuses rarely, ferns, coarse grasses, and a few fruit trees, especially the leiche, which is a beautiful plant, and, considering the soil, as well as the languishing appearance of other plants, grows with a vigour, and acquires a size, which is surprising." —Pp. 146—148.

Though Hong-Kong has the advantage of a rocky structure, it is not less insalubrious than the stations already noticed, of alluvial formation and swampy character. This the author ascribes to deserted rice-grounds, and to the presence of various ravines or gullies, containing brushwood and other vegetable products, which act here as they do in similar situations, in generating and evolving miasmatic exhalations.

Fever, especially remittent, was common on all the heights of the island; and it was extremely fatal. But flux appears to have been proportionately a more frequent disorder than at the former stations; and it was especially so among the crews of the ships in harbour.

In the course of November, as the temperature fell, fever became less frequent. But flux continued violent and prevalent, very intractable, and often fatal. Nine-tenths of the patients received into the Minden suffered from this form of disease; the subjects having laboured under it for weeks previously, or under it, alternating and combined with periodic fever. In the course of this time, whatever remedies were employed, great ravages were caused by the disease in the intestines; and at length many of these patients terminated their career in the advanced stage of the malady. It hence resulted that the period of highest mortality was not that of greatest sickliness, but took place at a more late period.

During the last week of November 1843, 146 seamen and marines were invalided and ordered to be sent home, chiefly in consequence of great and irreparable mischief produced in the intestines.

At length, after the commencement of January 1846, chiefly apparently from the lower state of the atmospheric temperature, the health of the squadron and troops began to improve sensibly. Dysenteric affections had disappeared; fever, and its connate disorder, flux, was uncommon; few new attacks took place, and those observed were mostly relapses or recurrences of former cases.

It appears in short that Hong-Kong, and especially the new town of Victoria, is a most unhealthy situation, and must for a long time prove singularly destructive to European troops and seamen. The author is inclined to listen to the representations of some of the colonists, who state that 1843 was a year of peculiar insalubrity; and that the ordinary course of a number of years would be found on the average much more healthy and less dangerous to the health of Europeans. It is nevertheless clear from his own observations, and above all from his account of the physical peculiarities and the hygienic features of its topography, that it must always be prolific in endemic fever, and its attendant malady, intestinal flux. Wherever we look, in short, it is the same mournful catalogue of evils and destruction. First, in general is periodic fever, commonly remittent; then that is succeeded by flux, variable in force, severity and persistence, but always causing much injury to the intestines, while the miasmatic poison is undermining all the powers of the frame; then come the indications of great mischief done to the system and the intestinal canal, pale, sallow, clay-coloured aspect, much emaciation and great debility; and, often as a final winding-up, dropsy and death.

Till after the 1st of June the force continued pretty healthy. It appears further that the year 1844 was much more healthy than that of 1843, the dangerous portion of the year, viz. from July to November, having passed with much less sickliness and mortality than the previous year. During the six months from May to October inclusive, the ratio of mortality in the Chinese squadron was as nearly as possible one per cent. of the employed. The proportion invalided during the same period was close upon 2 per cent., while that of sick varied from 4 to 8 per cent. of the employed. Of the patients sent to hospital 13 per cent. died. This was for 1844.

Comparing this with 1843, the ratio dying of the employed was 4½; that of the invalided, 7¾. The proportion of sick of the employed amounted sometimes to 20 per cent.; and of the number received into hospital, 33.3 per cent. died. It appears, therefore, that the number of sick was four times greater in 1843 than in 1844; while the mortality was exactly 20 per cent. higher in 1843 than in 1844.

It does not appear that it is possible to ascribe this great difference to any differences in the state of the weather, the temperature, the duration of the winds, or those telluric or meteorological causes, to which such differences are usually imputed. It is, therefore, left in that mystery and obscurity in which it is found. Dr Wilson, however, takes this occasion to read a sort of lecture of advice to those practitioners who complacently ascribe all recoveries to their own excellent and active treatment, all salubrity to their own care, attention, and foresight, and all sickliness and mortality to the ignorance, incapacity, and inactivity of their predecessors. "I cured that gentleman's sister," says Dr Lettsom, "of a most violent and dangerous fever." Alas! how little did this fashionable and celebrated physician know of the small assistance which even the most prompt and sagacious can render in curing fevers, and averting their fatal, or promoting their favourable termination. These exalted pretensions, and this vaunted superiority, Dr Wilson charitably and benevolently ascribes less to want of candour than inattention to, and consequent ignorance of, the dissimilar force with which the same disease acts at different times. In this there is some truth. But it is also impossible to disguise the fact, that there is a class of practitioners of such unbounded vanity and conceit, as well as of extreme and deplorable ignorance, that they never dream of such a thing as the force of disease being different, or the power of the constitution being capable of spontaneously curing the disease, with very little aid from the doctor, and probably better without his interference.

> "The more that is seen of some forms of disease, and the more closely they are studied, including especially the precipitous fevers of the tropics, the more likely is the conviction to come and deepen, that medicine has often but little remedial control over them; and grave questions will then arise as to whether artificial appliances, confused and contradictory as they often are, may not prove injurious rather than beneficial—if not curative or tending to cure, will they not become instruments of harm? Experience also shows, that while we think we are making rapid onward progress, we are often moving in a circle only, returning, after a long, toilsome, unprofitable journey, to the spot where our ancestors arrived long ago. This truth, which should humble and correct, not arrest us in the course of diligent, sober inquiry, meets us at every turn. One example out of many may be alluded to, namely, the treatment of dysentery in India, though that of fevers in general would not be less in point.

"After a variety of changes and revolutions, the most popular practice at present closely resembles that which was current sixty years ago, excepting the full animal diet and port wine, against which, at least the first, nature would sufficiently guard herself. It consists chiefly of opium, ipecacuan, and blue pill, with flannel; avoiding all strong measures, it seeks, by gentle means, to sooth interior irritation, and determine to the surface, endeavouring, according to the views of Sydenham, to turn the fever, which has fallen on the intestines, outwards and away from them. Different plans of practice are adopted there, as elsewhere, but this, according to a medical officer of standing, lately arrived at Hong-Kong from India, is the most in favour.

"To what decision should such things lead? Not to the abandonment or careless pursuit of improvement, certainly, but to make us more laborious and rigid than we too often have been in investigating the fitness of means before they are used, more vigilant in observing and scrupulous in recording them, and more certain that we do not parade, as new and useful, instruments which are old or worthless, or boast of their superior efficacy in cases unlike those in which they have been found unavailing by others."—Pp. 190—191.

We believe that the great thing wanted is to become acquainted with the natural history of diseases; to study them after the fashion and example of Hippocrates; and to ascertain how far they are spontaneously curable, and how far they present no tendency of this kind; to what extent the natural powers are capable of stopping, alleviating, or removing the phenomena of morbid action; and what are the cases in which diseases tend to disorganization, and what means we really possess of counteracting this tendency. It might be possible, then, to say how far the interference of the physician was really useful, and when it was to be unavailing or hurtful.

This subject leads naturally to the mode of treatment; and on this we find the author speaks with the sense and judgment derived from much observation, and long and careful experience.

In remittents, general blood-letting was practised to moderate extent, in a few cases, with advantage, in others without manifest effects, or even with detriment. Local blood-letting by leeches or by cupping was a better, safer, and not less efficient remedy. Blistering was of advantage after blood-letting; and it was used either over the epigastrium or over the scalp, or both, as the symptoms seemed to indicate.

Considerable confidence was placed in repeated small doses of calomel, viz. one grain every two hours, or in larger doses, as four or five grains after longer intervals, two or three times in twenty-four hours. After febrile symptoms were subdued, disulphate of quinine was employed, but often without good effect.

The system of giving the non-purging salts was not attempted;

and we wonder not, that it was not, for it is quite inadequate to the accomplishment of any good or great change.

Neither was the practice of throwing in calomel, as it is judiciously and appropriately named, thought safe; for the surgeons do not appear to have imagined themselves to be apothecaries' servants, or the human body to be a receptacle of mercury. Dr Wilson hesitates not to say, that of the two modes of administering this powerful medicine, viz. that by small doses, and that by large, the former is decidedly the preferable. The latter, he thinks, is invariably hurtful; and he expresses the opinion, that it must often have accelerated, rather than counteracted, the rapid sinking and collapse occasionally observed to ensue.

In the management of flux great difficulty was experienced. The intestinal disease it was often impossible to cure without first removing or stopping the ague. In cases where there was no lesion, arsenic was often highly useful; and it certainly, by curing the ague, enabled the medical attendant to give remedies for the flux.

Blood-letting was advantageously employed in acute cases, and blistering over the abdomen, as a subsequent measure, was very useful. Calomel and opium sometimes, with ipecacuan and antimony, were also found beneficial.

Purgatives, at least the irritating, we perceive the author dislikes, and we think properly. They were prescribed on an erroneous and unfounded hypothesis, and it is now understood that they did no good. In illustration of their hurtful effects, and of the beneficial results from treating the disease upon antiphlogistic measures, we think the following statement, which we are led to understand, is the case of the author at a former period, must be read with interest.

> "The disease, after a day or two of anorexia and intestinal disorder, accompanied by frequent liquid feculent stools, acquired its full dysenteric force in the harbour of Nassau. The patient was young and gladly availed himself of the aid of a senior professional friend, who kindly prescribed for him, but who had fixed notions respecting the right method of treatment, and insisted, fairly enough perhaps, on either having the entire direction of the case, or abandoning it altogether. This was felt to be rather a severe resolution, but under the depressing, and in some measure incapacitating influence of the disease, the method insisted on was followed. The symptoms ran high, corresponding closely with those enumerated above as characterising acute idiopathic dysentery; and the treatment was sufficiently simple, consisting of six-grain doses of calomel every fourth hour, and daily draughts of Epsom salts or castor oil.
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> "The misery of that week can neither be forgotten nor described, especially the amount of aggravation constantly occasioned by the purgatives. After each draught, the thirst, tormina, tenes-

mus, and muco-sanguineous dejections, were increased in force and frequency; but the offending feculent matters with which such war was waged were never expelled, for the sufficient reason, as the patient ventured to tell the doctor, that they did not exist. And the only change which the purgatives worked on the secretions was increase of quantity,—more frequent and painful discharges.

"While this was going on, a very simple process of reasoning led to the conclusion, which instinct strongly confirmed, that much benefit might be derived from blood-letting. It was felt, that by speedily unloading the vessels, and relieving the bursting vascularity of the mucous surface, the fire which was burning within, literally consuming the vitals, might be most effectually quenched. But the prescriber would listen to no such suggestions; he was satisfied with the efficacy of his own plan, considered every thing beyond it useless or hurtful, and looked upon the idea of the patient as the product of a visionary, perhaps diseased, disordered brain. Fortunately the calomel, unaided, and despite the purgatives, subdued the disease. Ptyalism set in; and the symptoms began to yield, and finally disappeared. But, though the method adopted was eventually successful, it does not follow that it was the best, or that it was not, in many points, radically wrong. Had blood-letting and the hot bath been employed, purgatives omitted, and opium with ipecacuan added to the calomel, there is no question that much suffering and danger would have been avoided. In three subsequent attacks, of at least equal violence at the onset, these measures were resorted to, and their curative agency was absolute and speedy. On each occasion, the disease was completely conquered in forty eight hours. This is impressive, and as far as it goes, certain instruction; and the writer has no hesitation in affirming that what he has felt in his own person, has been amply established by all that he has witnessed in others."—Pp. 211—213.

To the indiscriminate use of calomel in the treatment of flux or dysentery, Dr Wilson is rather opposed.

In the chronic form of the disease, and especially that which the author names anomalous flux, the chief remedies were calomel, opium, and ipecacuan in the form of pills; rhubarb; and magnesia, with a few minims of laudanum; Dover's powder; hydrocyanic acid; local bleeding by means of leeches; and revellents by means of blisters; and rubefacients by means of mercurial and iodine ointment rubbed over the abdomen.

In another variety of the chronic form of intestinal disease, manifestly approaching to, if not actually the lienteric, it was still more difficult to fix on an appropriate and efficient method of treatment. Mercury had little or no power over it. Small doses of rhubarb, with or without an alkali, and a few minims of laudanum, were administered in various tonic vehicles, as cascarilla, cinchona, or diosma. The latter became a favourite remedy, as it appeared to possess considerable remedial powers. The author recommends its use, therefore, not as a specific, but as a useful and beneficial agent, which seemed often to possess great powers in removing the symptoms and restoring health.

In the treatment of the ulcerative form of disease, Dr Wilson states, that the most effectual remedy by far, was incision of the part, so as to unload the vessels, to diminish swelling, allay heat and pain, and relieve the excessive tension of all the parts. The incisions were carried through the skin, and into the cellular tissue. The effect of this measure as an anodyne was so immediate and complete, that all other opiates and narcotics were abandoned. The appearance of the ulcer improved in proportion.

In the concluding chapter the author gives a sketch of the state of medicine among the Chinese. This is a matter of curiosity rather than utility; but it will be read with some interest; if for no other purpose than to shew the absurdities and foolish fancies, into which the human mind may wander, when under the dominion of superstition and ignorance.

In conclusion, we have only to say that in these Notes, and in a very unpretending form, the author has communicated a great deal of most valuable information. In many respects it is quite new, and drawn from a scene which has hitherto been to the English, as it were, forbidden ground. Where it is not new, it illustrates, enforces, and confirms principles derived from the experience of observers in other countries; and, if those whose interest it is, would study carefully the facts recorded and the observations made in this work, they would get rid of much ignorance and misconception, and they would be enabled to form correct notions on the nature and origin of endemic diseases in the countries bordering on the equinoctial regions.

Art. III.—*A Memoir on Amputation of the Hip-Joint, with a successful Case.* By William Sands Cox, F. R. S. F. R. C. S. England; Dean of the Faculty of Queen's College, Birmingham; Senior Surgeon of Queen's Hospital; Professor of Anatomy and Surgery at Queen's College, Birmingham, &c. &c. London, 1845. Folio, pp. 47. 2 coloured engravings.

Amputation at the ilio-femoral articulation is by no means a difficult operation. To a good anatomist it is a matter of comparative facility, and may be performed in a few minutes. But to the patient it is a very serious matter; a mighty and unequalled dismemberment; a mutilation so great that it inflicts on the constitution a shock almost irrecoverable; and in the most favourable cases the subsequent symptoms are violent and unmanageable, This has led many surgeons to recommend preferably amputation of the thigh through or immediately below the trochanters. This, however, is not all the evil attendant on it. From the nature of the diseases for which it is required, excluding injuries, it is rarely eventually successful. In most cases it is required by the presence of malignant, or at least suspicious disease of the thigh; and even after the operation has been successfully performed, and recovery has proceeded a certain length, the disease returns either in the stump or in the abdomen, and destroys the patient.

Mr Cox has met, like other surgeons, with a case in which this operation, in the meantime, has terminated favourably; and he has published the case, preceded by some surgical details, for the benefit of the profession.

It appears that the operation has been performed 26 times at least with success; that is, recovery of the patient from the immediate effects of the operation; and 58 times without good result. The total number of cases is 84.

The circumstances most commonly requiring the performance of amputation at the hip-joint, are extensive compound or comminuted fracture of the thigh bone, compound dislocation, removal of the thigh by machinery, mortification, caries and necrosis, exostosis and hyperostosis of the *os femoris*, and such tumours as osteo-sarcoma, osteo-steatoma, and spina ventosa.

Mr Cox, after noticing the methods employed by Malgaigne, Larrey, Lisfranc, Guthrie, and Beclard, recommends the following devised by himself.

The great objection, he thinks, to amputation at the hip-joint, is the difficulty of restraining the hemorrhage, and the shock given to the nervous system by so large an operation. The contrivance of the arterial compressor of Dr Segnoroni of Padua, he thinks overcomes the first difficulty, and the second is obviated or lessened by the rapidity of the operation by anterior and posterior flap.

The arterial compressor resembles in shape a horse-shoe. It consists of two blades connected in the centre by means of a screw and crank, by which contrivance the ends of the two blades to which permanent compressors are affixed, can be brought into perfect apposition, so as to check completely the arterial circulation when applied over a vessel, without impeding the return of blood by the veins. Previously to the application of the instrument, he recommends a small piece of wash leather or sheet caoutchouc to be laid on the integuments, to prevent the injurious effects of pressure on the skin; and to render the compressor more effectual, a piece of perfectly smooth soft cork about two inches long and one inch wide, with its under surface slightly grooved, or a pad of soft linen may be placed beneath the pad of the compressor.

The patient is to be supported in a recumbent posture, so placed that the tuberosities of the *ischia* project a little beyond the margin of the table. The compressor is applied over the external iliac artery, as it passes over the upper margin of the *os pubis*, and the thigh must be partially flexed. The operator is to stand on the outer side. The point of a narrow double-edged knife, about 12 inches long, is to be boldly and steadily introduced at one inch below the anterior superior spinous process of the ilium, and made to glide across the neck of the femur parallel with and a little below Poupart's ligament, so as to pass beneath the muscles of the anterior and internal region of the thigh, and beneath the superficial and deep femoral vessels, and be brought out about one inch below the margin of the anus. The knife is now to be carried downwards on a line with the anterior surface of the femur, from three to three inches and a half, in proportion to the bulk of the limb, and then brought obliquely downwards and forwards through the integuments, by which means the anterior flap will be formed. The flap is immediately to be thrown back, and its vessels, viz. the deep and superficial femoral arteries, at once compressed.

The limb is now to be depressed and rotated outwards, when the capsule being fairly exposed, is to be divided close to the edge of the acetabulum and next the round ligament. The head of the bone will then be made to slip from the socket by a slight rotatory movement outwards, and the knife is to be carried through the joint.

The posterior layer of the capsule, the extensor muscles of the hip, attached to the trochanter major, and the small rotators inserted into the digital fossa of the same process are then to be divided. The knife is to be passed downwards in a line with, and close to the posterior surface of the femur, for about three inches or three inches and a half, and afterwards downwards and backwards through the integuments, thus completing the operation. The posterior vessels are first to be secured, and afterwards the vessels of the anterior region.

The majority of surgeons recommend that the synovial membrane and cartilage of the acetabulum should be scraped off; but Mr Cox believes this to be a bad practice, and often productive of suppuration and disease of the bone. The cartilage and synovial membrane, he argues, will assume adhesive inflammation in the same manner as other soft parts, and the secretion will be suspended.

The nerves he recommends to be cut off as high as possible; because if they be involved in the cicatrix of the wound, they may be the seat of most violent pain, not only during the cure, but after the wound is healed.

The advantages of this operation he represents to be, 1st, Great security against hemorrhage; 2dly, The rapidity with which it may be performed; and, 3dly, The accurate manner in which the flaps may be united, so that they remain applied to each other as it were, by their own weight.

In this fashion the operation was performed in the following case.

Elizabeth Powis, sempstress, aged 23, was admitted into the Queen's hospital under the care of Mr Cox, on the 20th of July 1842. She had undergone amputation of the left leg, above the knee, at the General Hospital fourteen years previously, (1828,) in consequence of disease of the knee-joint. She stated that she remained in the General Hospital about two months, and that the stump was not quite healed when she was discharged; that about one month after this period, the integuments round the cicatrix ulcerated, and as the posterior surface of the stump substance formed, which was extremely painful; that the ulceration healed from the centre towards the circumference, but never were entirely cicatrized; that about six years after the operation, the integuments became hard and thick, and she experienced a sensation as if pins and needles were sticking in the stump; at length fungous growths showed themselves on the surface, and the parts became more and more painful; that about eight years from the date of the operation, she was again under surgical treatment, and was at intervals a patient at the dispensary and town infirmary.

In the Queen's Hospital she remained until the 13th November 1842, and during that period numerous topical remedies were applied, viz. lotion of dilute nitric acid, lotion of nitrate of silver, diluted liquor of diacetate of lead, liquor of iodine (Lugol); while tonic and alterative remedies were administered internally. From these little or no benefit was derived. On the 13th November 1842, she was discharged, and continued under the joint care of Dr Birt Davies and Mr Cox as an out-patient, undergoing treatment without advantage, until 1st July 1844, when she was re admitted.

At the time the stump presented the following appearances.

The integuments, extending upwards anteriorly for about three inches, and posteriorly for about four inches and a half, were of a dull white colour, and of cartilaginous hardness; and above this, for a limited extent anteriorly, posteriorly, and laterally, the same parts had a glazed corrugated appearance, like that presented by an old cicatrix. Patches of fungous growth, of a livid colour, protruded from a half to one-third of an inch from the general surface at intervals. From these excrescences blood occasionally exuded, and at times a sanious fluid. These excrescences were tender, and bled on being touched. The integuments of the upper part of the stump were healthy in character and feeling. There was no enlargement of the cutaneous veins, or of the inguinal or femoral glands. The stump generally was tender to the touch; the pain nearly constant, sometimes dull, and aching, at other times throbbing.

The disposition of the patient was said to be mild and placid, the complexion clear, the eyes and hair black; the chest was developed, and she was disposed to corpulence. She stated that her general health was good, that the catamenial periods recurred regularly every five weeks; that her father died asthmatic; and that shortly before his death a tumour, of what character she knew not, appeared in the arm-pit; that her mother died in a decline, as also her only sister; and that she has three brothers living who enjoy good health.

Topical remedies were again employed, and tonics and alteratives were administered without beneficial result, the fungous growths and morbid condition of the integuments continuing to spread. The patient is said to have become anxious for the performance of amputation; and as Mr Cox considered the case favourable for amputation at the joint, this was proposed and acceded to both by the patient and her friends.

On the 1st of November 1844, accordingly, in the presence of a number of physicians and surgeons, and fifty students, Mr Cox, assisted by Professors Knowles and Parker, operated by an anterior and posterior flap, this method affording the best means of restraining hemorrhage from the femoral and *profunda* arteries, and the formation of a good line of union. The existence of a patch of fungoid growth in the inner and posterior surface of the stump, within three inches of the anus, precluded the operation by lateral flaps, had such a mode been preferred.

The admeasurement of the parts, made a week previously, gave the following results, viz.

1. From the anterior superior spinous process of the ilium to the limit of healthy structure anteriorly, . . . 7 inches.
2. From the ischial tuberosity to the healthy limit posteriorly, ... not quite 4 inches.
3. Circumference of the thigh at the upper part, . 20 inches.
4. Transverse diameter of the same, . . . 7 inches.
5. Length of proposed line of incision, . . 10 inches.

The intestinal secretions had been regulated by medicine. The urinary secretion was natural.

At half-past 11 A. M., the patient was placed on the table, the nates slightly projecting over its edge; the horse-shoe compression was applied on the internal iliac artery as it passes over the body of the *os pubis*, so as completely to command the flow of blood through the arteries, without impeding its return to the veins, and placed in the care of Mr Parker. Mr Knowles had charge of the right leg. The stump was entrusted to Mr John Moore, resident medical officer.

The point of a narrow double-edged knife, 12 inches long, was introduced one inch below the anterior superior spinous process of the ilium, and carried across the neck of the femur, parallel with and a little below Poupart's ligament, beneath the muscles of the anterior region of the thigh, beneath the femoral vessels, and brought out about one inch below the margin of the anus. It was then carried downwards, in a line with the anterior surface of the femur, from three inches to three inches and a half, and brought obliquely downwards and forwards through the integuments. The flap thus formed was immediately thrown back, and the capsule of the ligament was exposed. The flap was retained and the vessels compressed by Mr Parker.

The stump being now slightly depressed and abducted, the capsule of the hip-joint was opened and rotated outwards; and depression being simultaneously effected, the head of the femur slipped without any force from the acetabulum. The *ligamentum teres* was divided, the posterior layer of the capsule, the extensor muscles of the hip and small rotators were separated from the *trochanter major*, and the knife passed downwards in a line and close against the posterior surface of the thigh-bone for about 3 inches, and then carried downwards and backwards, through the integuments posteriorly. The surface of this flap was immediately covered with a sponge. It being ascertained that the compression completely commanded the hemorrhage from the anterior flap, the posterior arteries, viz. two muscular branches, and the descending branch of the ischiatic artery were first secured. The screw of the compression being slackened, so as to show the mouths of the superficial and deep femoral arteries, the former vessel was secured and tied about three-fourths of an inch above its division, and the latter immediately after. All hemorrhage ceased. The operation was completed in less than thirty-five seconds. Not more than five ounces of blood were lost.

The flaps were placed in apposition, and the whole covered with the proper dressings.

During the operation the pulse did not fail, but towards the close of the dressing the patient became very faint, and was removed to bed in the horizontal posture. The patient had taken fully half a pint of port wine, yet continued in a very feeble state, with quick, feeble pulse, pinched features, pale lips, cold, clammy skin, and she complained of much aching pain in the hip-joint.

Cold weak tea was given, and warmth was applied to the feet; and at two, as pain continued, a draught containing 20 minims of henbane tincture was given, and directed to be repeated every third hour. At twelve at night it was found requisite to give 12 minims of the sedative solution of opium in draught. This was followed by sleep and abatement of pain.

On the 4th the dressings were partially removed, and the wound appearing natural, was again dressed. The pulse was 110; and the bowels had not acted, and medicine and enemata were administered.

On the 6th and 7th, though the pulse was 110, it was thought proper to allow soup on the former day, and solid animal food on the latter.

On the 11th November, in consequence of ulceration of the wound, and some hectic symptoms, disulphate of quinine was given, apparently with good effect. It is unnecessary to pursue the details. In January 1845, the patient was much recovered in strength, the wound slowly but steadily healing; and on the 5th February she was considered so well as to be able to be discharged cured.

In the summer of 1845, Elizabeth Powis had by the generosity of a benevolent lady enjoyed the benefit of two months' residence in the country; and she was then, on August 15, in robust health, and able, by her own efforts, to maintain herself.

We have only to express the hope that the cicatrix will not assume the indurating unhealthy action which was observed on that of the amputated thigh previously. This is so much the more to be looked forward to with alarm and apprehension, from the appearances found on dissecting and examining the parts removed. These appearances were the following.

The whole of the integuments inferiorly to the extent of about four inches were converted into an indurated mass of cartilaginous hardness, of pearly whiteness, from three-eighths to five-eighths of an inch in thickness. Under the microscope the structure appeared cellular and vascular, with myriads of minute globules, interspersed with spindle-shaped bodies, as observed by Muller in various tumours.

The cellular and adipose tissues were in considerable quantity, the latter remarkably dense and intersected with fibrous bands.

The muscles presented a peculiar granular appearance, were softened in texture, intersected with fibrous bands, and, with the exception of the muscles inserted into the trochanters, had undergone fatty degeneration.

The deep and superficial femoral arteries were not enlarged. The muscular and perforating branches, on being injected with size and vermilion, were found to terminate in a vascular network, distributed to the integuments and fungous growths.

The nerves terminated in bulbous enlargements. That of the sciatic nerve was as large as a walnut, grayish, solid, and vascular, and from it proceeded fibrous bands, which were lost in the adipose tissue.

The ilio-femoral articulation was healthy. Bony matter to the extent of one inch and a half, sharp, and pointed, had been deposited on the inferior surface of the femur.

These appearances and their indications are not good or favourable omens for the subsequent welfare of the patient. It is much to be feared that the same action and the same morbid changes may be formed in the parts constituting the new cicatrix.

The subsequent medical treatment of the case appears to have been well conducted.

ART. IV.—1. *Historical Details relative to the Military Force of Great Britain, from the earliest periods to the present time.* By HENRY MARSHALL, Esq., Deputy Inspector-General of Army Hospitals. (From the United Service Journal.) 8vo. pp. 113.

2. *A Historical Sketch of Military Punishments, in as far as regards Non-Commissioned Officers and Private Soldiers.* By HENRY MARSHALL, Esq., Deputy Inspector-General of Army Hospitals. (From the United Service Magazine.) 8vo. pp. 170.

WHEN we look at the great extent and power of the British empire, and consider the military establishments by which this power is maintained, it is natural to be impressed with the magnificence of the force, and its general aptitude for the purposes for which it is kept. Nor less are we taught to think of the patient endurance, the cool courage, and the invincible force of British soldiers in every situation in which these qualities are mostly required. Yet amidst the mighty and imposing results obtained, the condition of individuals is overlooked or neglected; the character and properties of the constituent parts are lost in the great movements of the machine; and amidst the well-contested fields and well-earned victories obtained by the exertions and prowess of the soldiers, the means by which they are made what they are, and the details of their condition, are either completely forgotten, or altogether unknown.

Yet soldiers are men subject to like passions, the same infirmities, and the same sufferings as the rest of mankind. No man becomes either much better or much worse for wearing a scarlet coat; and instead of being stronger or more healthy, they appear to be less so than other members of the human race. It is ascertained that the mortality is higher among soldiers, though a selected body of men, than among civilians, even were they not exposed to the vicissitudes of climate and season, and the casualties of warfare. In past times their condition has sustained much aggravation of misery and suffering from causes certainly not necessarily connected with it; and when we study the social economy and secret history of the British army, it seems not only astonishing that their achievements have been so great, but absolutely miraculous that they have done good at all.

The separation of a large number of men from the rest of the population, and the incorporation of these into an armed body, subject to particular rules and restrictions, is in a great degree an institution peculiar to modern times and modern nations. Among the states of ancient Greece, every man was liable to be a soldier or a sailor, at any period of his life, when the state required his services; and after one or two battles were fought—victory was ensured and peace was restored—the soldiers returned to their previous pursuits, and were lost among the general mass of citizens.

Among the Romans, in the early part of their history, every man not a senator appears to have been a soldier, or to have occasionally served as a soldier. We hear of no trades pursued by the Romans, except those of fighting with their neighbours or their enemies, or cultivating the fields as husbandmen, or keeping flocks as shepherds and neatherds. All the inhabitants of the district immediately round Rome appear to have served as soldiers, for a certain part of their lives, during the days of the republic. It appears to have been only after Augustus had consolidated the empire under his single arm, that a new system of military force was permanently and completely established. At later periods of the empire, when it was requisite to protect the frontiers from the assaults of barbarians, and maintain the provinces in tranquillity and subjection, the legions were recruited not merely from Italy, but from the different provinces of the empire; and the profession of the soldier was both honourable, lucrative, and not unfrequently rewarded by the possession of high authority, or even supreme power. The successive elevation to the purple of such men as Maximin, Claudius, Probus, and Diocletian, must have operated as powerful incentives to the ambition of the warlike barbarians, who were discontented with the humble and tranquil pursuits of herding cattle or cultivating the fields.

Among modern nations, though at their first origin the subjects were called on to perform military duties only for particular purposes, and for short periods, yet, in process of time, the multiplied inconveniences resulting from this mode of levy, led to the establishment of military bodies on more permanent principles. It appears, indeed, that the early English monarchs had three sources for the formation of a military force: 1*st*, The *posse comitatus*, or population from 16 to 60 of each shire, under the authority of the sheriffs; 2*d*, The quota of men which the holders of knights' fees were obliged to furnish; and, 3*d*, Such hired or mercenary troops as they might think proper or were able to engage. The *posse comitatus* could not be compelled to leave the kingdom, and might, except on certain occasions, refuse to leave the county in which they were raised; and hence the king could depend only on his feudal and mercenary troops when he resolved to carry an army to the continent, or when he had to wage war against the Scotch or Irish. Even the men raised in this mode to serve as soldiers might withdraw on the period of their service being completed, and return to their former occupations. No man in truth was by profession, and for a considerable portion of life, a soldier.

The first time that forces appear to have been raised and maintained for a considerable period as soldiers, was during the civil war between Charles I. and the Long Parliament; and many on both sides were doubtless enlisted from attachment to the cause, which they respectively espoused. In the year 1642, it is said that not fewer than 4000 enlisted in London in one day for the Parliamentary army.

In 1660, at the restoration of Charles II., the soldiers of the three kingdoms amounted to 60,000 men. Forty regiments were disbanded, and all that was retained was Monk's regiment, the Coldstream Guards, and a regiment of horse. In 1661, the Life Guards were raised from men who had fought as gentlemen in the civil wars, and the same year the Oxford Blues were embodied. To these corps were added the 1st, or Royal Scots; the 2d, or Queen's, raised in 1661; the 3d, or Old Buffs, in 1665; the Scotch Fusileers, in 1678; and the 4th, or King's Own, in 1680,—making a force of about 5000 men; and under the name of Guards these formed the standing army of Great Britain.

In the course of 20 years, the army of Great Britain was greatly increased. Soon after the revolution in 1688, it was found necessary to increase both the sea and land forces. In 1692, the House of Commons voted 32,000 seamen and 54,000 soldiers,—20,000 for the home service, and the residue for military service abroad. In 1697, however, the soldiers were reduced to 10,000; in 1698 to 7000, the seamen being 15,000; and in 1701 the seamen were raised to 30,000, and the soldiers to 10,000.

In 1702, the first year after the accession of Queen Anne, the House of Commons, which met on the 30th of October, voted for the purpose of carrying on the war, in conjunction with the allies, 40,000 seamen and 40,000 land forces, allowing for the maintenance of the last L.833,826,—L.350,000 for guards and garrisons, and L.70,973 for ordnance. At this time the population was 5,475,000, and the adult male population might be about half a million. In the battle of Blenheim, fought on 12th August 1704, the allies had 53,000 men, of which force, probably, at least 30,000 were English. These numbers were increased in the subsequent periods of the war.

In 1729, the House of Commons voted that the number of effective men be kept at 17,309; and though attempts were made almost annually to reduce the number of the standing force to 12,000, it was still kept at about 18,000, and was on certain occasions augmented. In 1734, the House voted for the service of the ensuing year 30,000 efficient seamen, and that the land forces should be increased to the number of 27,744 effective men. Yet were there almost annual debates against a standing army; and in 1738, it was declared by Lord Carteret and Lord Chesterfield in the Lords, and by various members in the House of Commons, to be the direct means of depriving the nation of its liberties, and increasing its debts to an insolvent amount. In 1740, the numbers voted were 40,000 seamen, and 30,000 for land forces.

At this time the population of England and Wales was 6,064,000; and the adult male population might be about 600,000.

In 1745, during the period of the rebellion, considerable difficulty had been experienced in sending a sufficient number of forces into Scotland to repel the Pretender, and at the same time defend the southern parts of the kingdom. Several British regiments were recalled from the Netherlands, and a requisition was made of 6000 Dutch auxiliaries; the merchants of London resolved to raise two regiments at their own expense; and orders were given to keep the trained bands in readiness, and to have the militia of the different counties ready to take the field. In consequence of these inconveniences apparently, and the apprehensions caused by them, the Commons voted for the service of the ensuing year 40,000 seamen, and about 60,000 land forces, including 11,500 marines. This appears to be the first time, subsequent to the restoration, that the standing army, or permanent force, arose to so great a number. In 1747 the numbers were much the same, viz. 40,000 seamen, 49,000 land forces, and 11,300 marines. In 1748, in consequence of the peace of Aix-la-Chapelle, the supplies underwent a reduction. The number of seamen voted was 17,000; the land forces 18,857, including guards and garrisons; the following year they were the same, while the seamen were lowered to 10,000; and next year (1750) to 8000.

At this rate nearly the army appears to have continued till 1755, when, in consequence of our losses in America, and our military operations there, they were at once increased to 50,000 seamen, including 9138 marines, and 34,263 land soldiers; and in a few months it was deemed necessary to raise besides 10 new regiments of foot, and 11 troops of light dragoons, making in all an addition of 6600 men, or rather more than 40,000 soldiers.

At this time the general population amounted at the lowest to 6,500,000; and the adult male population must have been about 700,000.

A custom had become common since the accession of George the First, to employ, besides British forces, various bodies of German soldiers, Hanoverians, Hessians, Brunswickers, and others; and the number of these troops thus kept varied from 30,000 to 50,000. These were mostly employed abroad in the German dominions of the Elector of Hanover.

Between 1755 and 1761, the numbers of seamen, including marines, and soldiers voted annually for the public service were as follow:—

	Seamen & marines.	Soldiers.	Battalions on Irish Estab.	Foreign troops. Hanover.	Hessian.	Population.
1755	50,000 M. 9138	34,263				
1756	55,000 M. 11,419	49,749				6,500,000
1757	60,000 M. 14,845	53,777				
1758–9	60,000 14,845	52,533	5	50,000		
1759–60	70,000 18,355	57,294	4	38,750		
1760	70,000 18,355	64,971	3			6,736,000

At this period, and for many years previous, there was always a large body of militia and volunteers; and considerable sums were voted either occasionally, from time to time, or annually, to pay this militia force in the different counties of England, and the regiments called fencibles in Scotland.

It is unnecessary to trace the cause of these gradual and successive elevations further. It is sufficient to say, that by this time it was understood and admitted, that a permanent military force, or a body of men, by profession soldiers, for the whole, or the greater part of life, was regarded as necessary either to maintain the internal tranquillity of the kingdom, to repel or prevent the attacks of foreign powers, or to maintain peace and sufficient authority in our colonial establishments. This force was never below 40,000 men after the period referred to; successively it amounted to 50,000, 57,000, and 65,000. At a subsequent period, viz. in 1774, it was calculated by Henry Home, Lord Kaimes, that a body of 60,000 men would be required for the exigencies of Great Britain, even supposing the country to be in a state of profound peace. It seems extraordinary, that this learned lawyer was not aware, that even in 1760 it had risen much above this number; and in 1774 it must have been above 75,000 men. Previous to July 1760 it was ascertained that 33,000 men had been raised in Scotland for the public service by sea and land.

Subsequent to this period the number of effective soldiers maintained by Great Britain rose to a much higher amount. Near the close of last century, viz. 1795, during the revolutionary war, the regular army, including the force employed in North Holland, consisted of 119,000, besides about 42,000 employed in the colonies, in Gibraltar, and in Portugal; making a total force of 161,000 men. In 1808 the British army consisted of 30,000 cavalry, 6000 foot guards, 170,000 infantry of the line, and 14,000 artillery, making a force of 220,000. Of this number between 50,000 and 60,000 were on service in the colonies. There was besides a force of 37,000 marines, and at least 80,000 militia; the latter being engaged in home service.

In 1813, the military force of Great Britain amounted to 227,442, besides 28,000 in the East Indies, and exclusive of 93,210 embodied militia. At this time the general population of Great Britain was about 13,000,000.

At the battle of Waterloo, in 1815, it is calculated that the British force, including the German legion, under the Duke of Wellington, amounted to 36,487 men. At the beginning of the campaign they amounted to 43,236.

After several years of peace the numbers were diminished. From 1826 to 1829 the strength of the army was 111,000 men; and from the latter period it has remained at a little above 103,000 men.

How was this force raised and kept in order, so as to be efficient for the purposes of the public service, is a natural and not unimportant question. What were the means by which it was kept in subordination, in discipline, in health and energy, and, in short, in that state which is necessary to the useful employment of large bodies of armed men, in enduring military labours, overawing enemies, obtaining victories in battle, and fulfilling in all respects the objects with which they were enlisted and armed?

The answers to these questions must suggest any thing but agreeable reflections.

It appears that in obtaining recruits to be soldiers, every method implying the most unlimited use of force and fraud was employed. So early as the reign of William III., it was a matter of considerable difficulty to find men to complete the requisite numbers required to serve as soldiers, and various illegal methods were resorted to for the purpose.

In 1692, a formal complaint was presented to the House of Commons by several members, that their servants had been kidnapped and sent to serve as soldiers in Flanders. A committee was appointed to inquire into the abuses committed; a remonstrance was presented to the king, who caused inquiry to be made; but this proving ineffectual, a great number of persons who had thus been pressed were discharged by the House, and Captain Winter, the chief undertaker for this method of recruiting, was carried by the Sergeant before the Lord Chief-Justice, that he might be prosecuted according to law.

It does not appear that much good resulted from this proceeding, beyond the establishment of the fact of illegal methods being adopted to induce men to become soldiers. The difficulties continued all the subsequent reign; and no proper attempt was made effectually to remove them. A set of low agents were employed in towns and other places where men might be abundant and idling,—in order to inveigle men by every means into the hands of recruiting serjeants and recruiting officers.

There are good grounds for believing, that absolute man-stealing or kidnapping, either for the army or for supplying the colonies with servants, was largely practised in various towns; at least such places as Newcastle, Bristol, and Liverpool, then but a small place, enjoyed an evil reputation in this respect; and the practice had evidently extended to Scotland.

In 1740 the system of kidnapping had extended to Aberdeen, and was there practised probably for several years previous to that date. At all events, it was proved by a person named Peter Williamson, who afterwards established the penny post in Edinburgh, that many young men had been enticed or rather kidnapped, and sent to the colonies as slaves or soldiers, and then measures were adopted to prevent them either from returning or communicating with their friends. This man, however, had contrived to elude the vigilance of their agents, and returning in 1758, exposed the whole system. It was then only that this sort of slave-trade was stopped.

It was consequently the practice sometimes to deceive unwary and inexperienced youths; at other times to cajole agricultural servants and discontented apprentices or tradesmen, by representing to them the delights of living an idle gentleman-like life, killing Frenchmen and becoming rich by booty; and when these modes were found inadequate, it was not unusual to entice raw lads at fairs into drinking-booths, there intoxicate them, and then by tendering them the shilling or earnest-money, to assure them, when they had slept off the drunken fit, that they were her Majesty's servants. When these means were insufficient, as was almost constantly the case, then the gaols, prisons, and bridewells were emptied, and a multitude of men, some of them guilty, others perhaps thoughtless and imprudent, all more or less doubtful in character, were gladly received as recruits in the different regiments whose numbers were deficient. From the testimony of Defoe it appears that the Mint, a noted receptacle of debtors, and Alsatia, an equally noted receptacle of ruffians, were often allowed to contribute their proportion of inhabitants to fill vacant ranks in the army. The object of the vagrant act, indeed, as it is termed, and which was passed in the reign of Queen Anne, 1704, was to provide for the army, by compelling all persons who might by any means be classed under this suspicious name, to enter the army or be punished as vagrants; and this act was thought a great improvement on the old methods of cajoling and entrapping unwary young men by drinking, by falsehood, and misrepresentation. Vagrants and similar persons were also often compelled, in order to escape imprisonment, to enlist, and so late as 1779, men of this stamp were legally pressed to serve in the army. The mode, in short, of recruiting the army, was quite similar to that which took place, of supplying crews for ships, described by that renowned and veracious person, Lemuel Gulliver, and it accordingly produced much the same effects. "They were fellows of desperate fortunes, forced to fly from the place of their birth, on account of their poverty or their crimes. Some were undone by law-suits; others spent all they had in drinking, whoring, and gaming; others fled for treason; many for murder, theft, poisoning, robbery, perjury, forgery, coining false money; for committing rapes, for flying their colours or deserting to the enemy, and most of them had broken prison. None of these durst return to their native countries for fear of being hanged or starving in a jail, and therefore they were under a necessity of seeking a livelihood in other places."

That this contained no great exaggeration, may be inferred not only from what has been stated already as to the object and operation of the vagrant act, but also from the observations of Bruce, who describes the moral character of European armies during the early part of last century in the following terms. "We are not now scrupulous in admitting unfit persons to be soldiers; for instance, if all infamous persons, and such as have committed capital crimes, heretics, atheists, and all dastardly and effeminate men; if all these, I say, were weeded out of the armies that are at present a-foot in Europe, it is much to be feared that most of them would be reduced to a pretty moderate number."

When the methods employed were so bad, the results could not be good. A few respectable or not very bad men might be found in a regiment, but the majority were supposed to be, and often were naught, and for every useful purpose worthless. In truth, many persons scrupled not to look on soldiers as the outcasts of society, men sullied with vice and debauchery, often stained with crime, and therefore to be ruled and kept in order only by means the most forcible and violent. "The greatest part of the soldiery," says the author last quoted, "being men of so ignoble disingenuous tempers, that they cannot be made obedient by the allurements of rewards, nay coercion being, generally speaking, the surest principle of all vulgar obedience; there is therefore another part of military institution, fitted to such mean capacities, and these are the various punishments awarded to their crimes, which as goads may *drive* these brutish creatures who will not be *attracted*."

Here is the acknowledgment of a regular system of punishment in order to compel about 40,000 or 50,000 men to perform the duties expected of them,—to live soberly, temperately, and correctly; no attempt to make them understand their errors and reform them; none to enlighten their minds, or show them that their true interest was to perform their duties regularly, and to abstain from all vice and crime, but a mere system of force, terror, and incessant punishment.

> "It will readily be conceived," says Mr Marshall, "that an army which was more or less recruited by felons and pressed vagrants must have ranked very low in the scale of morality, and required to be kept under very strict discipline; and it may be inferred from the harsh inhuman manner with which Bruce speaks of the men, and also from the usages of the army at the time, that the penal mode of reformation was not spared. But it ought to be recollected that excessive punishments are apt to render men desperate, and to lead to more dangerous crimes than those they are intended to prevent; and consequently I do not suppose that the army was materially improved by the violent coercive measures employed."

Certainly it was not. Yet this was one of the most brilliant periods of the military history of Great Britain. It embraced such great victories as Blenheim, Ramillies, Oudenarde, and Malplaquet, besides numerous minor actions, in which the courage and discipline of English soldiers were conspicuous. This reprehensible and unjust system of procuring soldiers and recruiting the ranks of the army was continued for a very long period, and always with very much the same effects on the character of the troops. The result was, that not only was the profession of a soldier regarded as that of an armed mercenary ruffian, but the army itself was so unpopular that it was becoming daily more difficult to recruit its ranks. In various regiments the men deserted, or fell on sundry expedients to render themselves unfit for service. Many counterfeited sickness, infirmity, or lameness; others maimed themselves; others committed suicide; and it may be said indeed that they were constantly committing a sort of slow suicide by the practice of drinking to excess ardent spirits and other intoxicating liquors, whenever or wherever they could procure them, and which was invariably held out to them as an inducement to enter the service, and which they were, when in it, encouraged to practise to as great an extent as was compatible with their means and the power of performing their duties.

Notwithstanding, however, the experience of 40 or 50 years of this hurtful method of recruiting the army, it was continued, and in several respects formally renewed in 1744 by a renewal of the vagrant act of 1704. This must have been not only hurtful to the army itself, but enabled unprincipled magistrates and others to exercise great tyranny and oppression on their fellow-subjects. Constables and churchwardens were to be employed in searching for and securing able-bodied men, and they were to receive a reward of one pound for every man pressed, and 6d. for every day they kept a man in confinement, and to be fined L.10 for every wilful neglect in securing a man—half to go to the informer. Here is a system of man-catching established by the legislature in the freest nation in the world.

The natural effects of this measure were a general crusade against all and sundry able-bodied men, who were speedily carried to the various gaols and prisons, and there detained until examined, and finally enrolled with some corps. It is said that on the 20th April 1744, upwards of 1000 men were secured in the jails of London and Westminster.

The unscrupulous mode in which the act was carried into effect against individuals may be understood from the following story, related by Mr Marshall, from the "Life and Times of the Countess of Huntingdon:"—

"In 1744, the vicar of Burstall thought it justifiable to rid the parish of a man who preached with more zeal and effect than himself, and lent his assistance to have the man pressed as a soldier. The man was brought before the commissioners at Halifax (where the vicar was on the bench,) who refused to hear him plead his cause, saying, 'We have heard enough of you from the minister of the parish.' 'So, gentlemen,' said the preacher, 'I see there is neither law nor justice for a man who is a Methodist.' And addressing the vicar by his name, he said, 'What do you know of me that is evil?

Whom have I defrauded? or where have I contracted a debt I cannot pay?' 'You have no visible means of getting your living,' was the reply. He was forthwith marched off to Bradford, and confined in a dungeon, where there was not even a stone to sit upon."

This act was renewed in 1745, when hernia or rupture is first specified as a disqualifying infirmity, although distempers and infirmities in general are mentioned as objections.

Hitherto the service was in general for life; and this was often perfectly safe for the government, while it was extremely injurious to the service. The soldiers, indeed, served till they were killed by the enemy, died of disease, then frequent and destructive, or were disabled by wounds, or cut off by their own intemperance. It must have been a rare occurrence for a man to live so temperately, or be so fortunate as to serve until he was deemed unfit for further duty. In 1749, an attempt was made by a new bill to limit the period of service, so that every soldier might claim his discharge, after serving ten years, upon payment of L.3. This was lost, and the unlimited service system, with all its evils, continued.

It might be imagined that when men were procured with such difficulty, great pains would be taken to make them good soldiers, and they would be highly valued in proportion to the difficulty with which they were obtained. This does not, however, appear to have been the case. Although every soldier must have cost a good deal of money, he seems to have been valued at a very low rate. No pains were taken to instruct or train him except in the use of his firelock and bayonet, and the art of mere shooting and stabbing. He was allowed to live the most irregular life if he attended his drills properly. He might drink as much and be drunk as often as he could find money for it, providing he could always appear sober or even steady on parade. Thieving alone was regarded as an unmilitary crime, and that was punished with the greatest severity. But all other violations of the moral law were permitted; and these were by no means unfrequent among men so ignorant, so unprincipled, and so degraded as soldiers actually were, or were often permitted by their superiors to be.

Of this system it was a necessary part that the only method of keeping the soldiers in order was coercion, restraint, and punishment; and in general these were excessively severe and cruel. This cruelty was doubtless borrowed from the criminal law of the country, in which the punishments were cruel, and totally disproportioned in many instances to the offence, and upon these the system of military punishment was an improvement. The punishments were either capital or affecting the life of the individual, or affecting his person, his liberty, or his character.

Capital punishments in the army consisted either in shooting the individual or hanging him; and the first was always performed with every circumstance which could make death by that means cruel to the sufferer and terrible in the eyes of the beholders. An execution party was named, consisting, when they could be obtained, of hardened deserters, or persons guilty of the same crime for which the party was to suffer; or, if there were no men in these predicaments, the party was taken from the men next for duty, and a steady non-commissioned officer was appointed to command them. The prisoner was then brought from the provost-marshall, with his hands tied behind him, attended by the chaplain of the regiment, and followed by the execution party. In this manner he was led along the line, guarded by a detachment of the regiment to which he belonged, or by one from the pickets of the army. On his arrival at the place of execution, after some short prayers, a cap or handkerchief being drawn over his eyes, he knelt down, and the execution party making ready and advancing within six or seven yards of him, on a signal given by the criminal, two-thirds fired; and if he should not be killed, the reserve stepped up close, and put him out of pain. All the troops then were made to march by the body, that by example they might be deterred from a like offence. The recruits, in particular, were made to file close to it. This was the practice previous to 1786.

At a subsequent period another ceremony was added to this sanguinary proceeding. After the criminal was declared by the attending surgeons to be dead, the mangled corpse was carried three times round the parade, or place of execution, in order to render the example more striking, and to impress the greater horror on the minds of the spectators.

Whether this punishment, and this mode of inflicting it, really answered the purpose intended, of checking crimes, we are not informed. Judging from the results for 40 years after, there is reason to think that it did not. One effect, however, there can be no doubt it must have produced, that of wounding the feelings of good men, and brutalizing the minds of the soldiers in general, and rendering them callous to the greatest cruelties, and capable of committing the most atrocious acts, even homicide and murder itself, in wantonness and cold blood. We have heard of soldiers, trained in this school, who in foreign countries, and in the colonies, did not hesitate to shoot an inoffensive peasant, merely because he was a good mark.

Capital punishments, by shooting or by suspension, were by no means sparingly used, especially on foreign service. But these were gentle and humane compared with the practice of flogging, which afterwards crept into the army and became general. It is not accurately ascertained at what time this form of punishment was introduced into the army. Originally, it appears, that officers had the power of whipping the soldiers and causing them to be whipped for certain offences. Flogging is not mentioned in the treatise of Bruce, published in 1717. The ordinary criminal law of the country was at that time not remarkable for mildness; many offences of very different degrees of criminality being then punished indiscriminately with death. Accordingly, the majority of punishments in the army, during the reigns of George I. and George II., appears to have been death. Thus death was inflicted on men found gathering pease or beans in a field. Whipping, however, appears gradually to have come into use previous to 1761, if we believe Grose, about 1756; and flogging is first mentioned by Colonel Dalrymple, who published in 1761, as a mode of punishment, cruel and degrading, but inefficient. In an Essay on the Art of War, published in 1762, the recognised modes of punishment for crimes are stated to be, to be hanged, shot, sent to the galleys, chained to a wheelbarrow, or to run the gauntelope; while for faults the punishments were, the wooden mare, picket, imprisonment, chains, and bread and water diet. Whipping and flogging are not mentioned; but this may be implied in the punishment of running the gauntelope. This consisted in making the criminal walk or run uncovered the whole length of the line of his regiment, drawn up two deep, while every man, previously provided with a willow, struck him on the back, shoulders, or sides, as he passed. After this time it appears to have been not unfrequent, though not altogether a recognised mode of punishment. In July 1763, at the Winchester assizes, a special jury was sworn to try a cause, in which George Dawson, late a soldier in the 85th regiment, was plaintiff, and three lieutenants and three drummers were defendants. The action was brought for trespass and assault, and false imprisonment of the soldier. In the course of the evidence it appeared that one of the defendants had caned and imprisoned the plaintiff without just cause, and that the plaintiff had received at the halberts 300 lashes with a cat-of-nine tails, under colour of a sentence of court martial, of the proceedings of which no evidence was given by the defendants. After a long hearing, the jury found a verdict for the plaintiff, with L.300 damages; L.200 against one lieutenant, and L.50 against each of the other two.

Running the gauntelope was a punishment irregular in its infliction and effects; and it is said that for this reason it was abandoned; and punishment, by inflicting a fixed number of stripes, or scourges, or lashes, on the culprit, secured in a particular manner, was substituted in its place. At all events, it became gradually less frequent, while flogging became universally established; and, in 1805, it is said that running the gauntlet was completely unknown in the British army.

In establishing scourging or flogging as a method of punishment among soldiers, not the slightest attention appears to have been bestowed on the question, how much or how little could be with safety, or rather without permanent injury, endured. The individuals who fixed the amount of infliction were not to endure it, and that was quite enough. They were not satisfied with making a fellow-creature endure a disgraceful and ignominious punishment—a punishment, cruel and revolting to the human feelings, but they ordered a number of stripes to be inflicted, which was worse than death; and which, if inflicted altogether, must have often caused death, as this in many instances actually did. The usual amount of stripes, scourges, or lashes, as they are called in the military style, awarded was 500, 800, 1000, 1200, and so forth; and it is mentioned that George the Third was the first individual who, so late as 1808, doubted the propriety of awarding more than 1000 stripes. It is singular that the gracious monarch did not think that even 1000 stripes were greatly more than ought at any time, or for any offence, to have been inflicted. Russia, it is said, boasts that capital punishments are unknown in her criminal code.* But what is the fact. The criminal, though not condemned to death by law, is often in fact. He is sentenced to receive a certain number of blows with the knout; and it is very easy for a strong and dexterous executioner to kill the criminal at a certain number of blows; and very often the criminal was actually killed outright by this instrument. So is it with the system of flogging. Though it be supposed that it is a punishment short of death, yet in many instances it terminated fatally; in all it impaired much the strength of the sufferer, and generally rendered him much less able than formerly to perform his duties, or endure the fatigues of the service. The stories related in the present tract of the mode in which the punishment was inflicted—the faults or errors for which it was awarded, and the extent to which it was sometimes carried, show that no human being is capable of such cruel and inhuman, not to say unjust and oppressive conduct, as man in authority is to those under him. Had the officers who awarded this punishment, or the legislators who sanctioned it, been made occasionally to feel it, they must have learnt practically, not only its degrading influence, as a mere corporeal infliction, but the hurtful effects which it produced in shocking the feelings, breaking the spirits, and destroying the self-respect of the soldiers.

A great clamour, and we admit that it was just, was raised in the ears of the people of England, about the treatment of negro slaves in the West Indian Islands; that they were scourged, beaten, and sometimes killed by their cruel task-masters and proprietors, and that the whole was a system of cruelty unbecoming the human race, much more a nation professing the Christian religion. It is a fact established by the most incontestable evidence, that long before this complaint was made, and while it was annually served up to the English nation, a more cruel and execrable system of oppression, torture, and wanton barbarity, was carried on systematically and regularly in the British army, upon persons born free, natives and subjects of England, and supposed to be living under the protection of its laws. The scourging of the negroes was a mere trifle to that of the soldiers; trifling in its extent and frequency compared with the frequency with which this punishment was inflicted in the army. Upon no negro could more than 40 stripes be inflicted even by a magistrate. The task-master of the negro was in general deterred from carrying his punishment too far, by the fear of losing the services, and lowering the value of the negro. But the commander of a British regiment of whites was under no such feelings and restraints. He did not pay for the soldier as the West India planter did. Of this burden the public purse relieved him. He was convinced, whether right or wrong, with or without reason, that the only mode of governing soldiers was by terror, coercion, and every species of torture; and many never took the trouble to put the question to themselves whether this view was correct or not.

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* Capital punishments were abolished in Russia by the Empress Elizabeth. By means of the use of the knout, however, they were in fact retained.

As to the effects of the infliction of the sentence of 1000 stripes, the amount beyond which George III. thought it unsafe to go, facts were by the practice of the army abundant. It seems only singular that any person, knowing what the human body is, could have imagined that anything near this was a safe infliction. A case from the experience of the late Dr Fergusson places it in a very clear light. A marauding soldier of the 5th regiment in 1799, then stationed at Silver Hall Barracks in Sussex, had been guilty of breaking into a farm-house at night, had wounded with his bayonet the woman of the house, and committed other outrages, for which, Dr Fergusson thinks, he ought either to have been given over to the civil authority or hanged before the assembled troops. He was sentenced to receive 1000 lashes. The first part of the punishment he bore well, being a strong, robust, well-made man. Seven hundred and fifty lashes were most severely inflicted, when Dr Fergusson, perceiving from his countenance alone that nature was giving way, had him taken down and carried to hospital. In a few weeks he was reported cured, and the commanding officer declared that the sentence should be inflicted to the utmost lash. He was again brought out. It was winter, and snow was on the ground. He was tied up with his back to the wind, and punishment began.

"At the first lash the newly-organized skin gave way—the blood streamed down his back—and he who on the first infliction was all defiance, writhed and screamed out. As the flogging proceeded, the lash became clogged with blood, which at every wave of the drummer's arm was driven in showers over the snow-covered ground. His cries became yells, and the integuments of the newly cicatrized back were cut literally in pieces. I stopped the punishment when he received 60 lashes. But his second cure was now a very different affair. Healthy suppuration could not be established after such reiterated injury, and sloughing and deep-seated abscesses were formed among the great muscles of the back. When I left the regiment, on promotion some months afterwards, he was still in the hospital, a poor, hectic wretch, utterly broken down from the effect of the second flogging. I never learned whether he ultimately recovered. Any summary form of death that could be inflicted would have been no more than such a ruffian richly deserved; but in a Christian land, not such a death as appeared to be impending when I came away."

It appears subsequently that all second punishments, after the delinquent has borne as much as he could endure at once, are illegal; and consequently this man, bad as he was, was unjustly punished the second time.

One of the first absurdities that strikes an observer is, the disproportionate nature of the offence to the punishment which is awarded for it. That a soldier be scourged with 100, 200, 500, or 800 stripes, it is not enough that he be guilty of a crime, in the ordinary acceptation of that term. Military offences or delinquencies are estimated and determined by a balance of a peculiar kind. If a man's queue dropped off, (p. 39,) if his shirt was not quite clean, (40,) if he coughed in the ranks, if he brushed a fly from his face,—all these were regarded as enormities requiring the infliction of stripes and scourging to the extent of denuding his back of skin. It is stated by Serjeant Teesdale, that of one regiment stationed at Bremen in 1806 for about six weeks, the officers received orders from the major to inspect the men closely, and turn to the front all such as were found dirty. A square was then formed for punishment, and the men found not clean enough were marched in, tried by a drum-head court-martial, and flogged to a man. It is added, that from 10 to 15 and 25 soldiers were daily flogged at parade on this pretext. The practice was at length forcibly stopped by the orders of a superior officer. Officers such as this major are totally unfit to command a regiment or even a company of soldiers.

But what must be said of the character of the gentleman and his fitness to command, who punished a man under the following circumstances?

"Major B——," says Mr Marshall, "while he commanded the African corps, a corps always notorious for corporal punishment, was one Sunday reading the morning service of the Church to the men, who were formed into a square. The major, who was from the north of the Tweed, spoke and read the English language with the broad accent of the natives of one of the counties of the north of Scotland. Upon reading the Creed and pronouncing the words, "Suffered under Pontius Pilate," in his own queer way, a wag in the ranks, well known for his uncontrollable propensity to joking and fun, exclaimed, "Wha's Ponshews Peelate, I wonder?" The major paused, and laying down the prayer-book, said, "Ah John, is that you at your jokes again? Just come out here, my man." The soldier stepped forward; a drum-head court-martial was held; the triangles rigged out; and John received one hundred lashes without saying a word. The flogging having been completed, the major resumed the prayer-book, and finished the service of the day."

One is tempted to ask what was the religion of this military apostle, and what was the use of his reading the service at all? It would have been too much to expect such a man to give an explanation of who Pontius Pilate was. But he might have recommended any one wishing to know, to read over in any of the evangelists, the account of the scene in the audience chamber of the house of the Governor of Jerusalem. Conceive St Paul, or St Barnabas, or St James, stopping to order a man to be tied and scourged amidst divine worship. Such persons as this should not be permitted to hold the command of a regiment one single hour after such a transaction.

The palmy season of corporeal punishment in the army it is not easy to ascertain. It is probable that at the close of the last century it was extremely prevalent. It is certain that, for the first six or eight years of the present century, it was carried to a most extreme extent, both in frequency and in amount. Major Macnamara states, that it is scarcely an exaggeration to say, that during the war at least three-fourths of the soldiers of almost every regiment in the service had felt its sting; that is, supposing the force to consist of 220,000, 165,000 men had been scourged and flogged.

Another feature in this system, equally disgraceful to the judgment and the feelings of the governing and executive parties, was the employment of a medical officer to superintend the punishment, to see how far it might be carried without permanent injury, and, in short, to determine the limits of human endurance under a barbarous infliction. It has been generally supposed, that it was the duty of medical men to relieve pain and suffering, to prevent intentional injury and accident, to counteract injury, and to cure any effects of accidental injuries. Here he is made a coadjutor in the infliction of a most severe injury or artificial disease, an injury which often did terminate fatally, and the bad effects of which no medical knowledge could foresee—no medical management could modify or prevent; in short, the medical officer was degraded by being converted into an assistant to the provost-marshall and executioner. It might justly be argued, that if punishment was to create an enfeebling and protracted disease, rendering the soldiers disabled and inefficient, why have recourse to such a method, when it was well known that it often required all the care and foresight of the medical officer to ward off the attacks of natural disease, or alleviate and counteract them, when they took place. If a soldier was so difficult to be obtained and so expensive a person, why take so much pains and trouble to destroy him.

It is no small aggravation of the whole of this system of government and discipline, that it did not answer the objects with which it was administered.

> " The frequent infliction of corporeal punishment in our army," says the Hon. Brigadier-General Stewart, 95th regiment, so early as 1806, " tends greatly to debase the minds and destroy the high spirit of the soldiery; it renders the system of increasing rigour necessary; it deprives discipline of the influence of honour, and destroys the subordination of the heart, which can alone add voluntary zeal to the cold obligation of duty. Soldiers of naturally correct minds having been once punished corporally, generally become negligent and unworthy of confidence. Discipline requires the intervention of strong acts to maintain it, and to impress it on vulgar minds. Punishment may be formidable but must not be familiar; generosity or solemn severity must at times be equally recurred to; pardon or death have been resorted to with equal success. But the perpetual recurrence to the infliction of infamy on a soldier by the punishment of flogging, is one of the most mistaken modes for enforcing discipline that can be conceived."—P. 15.

It appears in accordance with these sentiments, that it was long after observed, that the best regiments in the service are those in which flogging was discontinued. Yet we are informed, that the mean number of lashes inflicted monthly in one regiment in 1808, serving in India, was for some time 17,000. Mr Guthrie mentions in his Lectures, the case of a man upon whom had been inflicted in the course of his service 15,000 lashes.

At length by the force and weight of public opinion, men in office began to think that there might be, in the practice of flogging, something which required consideration,—something, in short, wrong, and requiring amendment. It was in the navy, however, that the first indications of doubt and inquiry appeared. By an order from the Admiralty, dated April 1811, a quarterly return was directed to be made to the Board, of the number of seamen and marines flogged in each of his Majesty's ships, with their crimes, age, time they were confined, and the amount of punishment, leaving it, however, in the power of the commander or captain to inflict punishment, whenever he shall deem it absolutely necessary for the good of the service.

The same year, when the mutiny-act was brought before Parliament, a new clause was introduced, proposing to empower courts-martial to imprison instead of inflicting the penalty of flogging.

On the third reading of the mutiny-act on the 13th March 1812, Sir Francis Burdett proposed a clause, forbidding the cruel and degrading practice of flogging. A division took place, when 6 voted for the motion and 79 against it. On the 15th April 1812, a motion was made in the House of Commons by the Hon. H. Grey Bennett, for a return of the number of corporal punishments inflicted in the regular army, militia, and local militia for the previous seven years, distinguishing the number of lashes in each case, and the crimes for which they were inflicted. For this motion there were 17 votes, against it, 49.

Public opinion nevertheless was doing its work. On the 25th of March 1812, a circular letter from the commander-in-chief was addressed by the adjutant-general to the officers commanding regiments, giving the following directions; 1st, Restricting the authority of courts-martial to the purposes for which they are designed by the legislature; 2d, The commander-in-chief's order, that on no pretence whatever, the award of any court-martial hereafter shall exceed 300 lashes; and, 3d, Recommending attention to prevent rather than to punish crime, and to avoid all unnecessary severity.

That such an order was indispensably necessary may be inferred from the fact, that a very large proportion of officers held the decided opinion, that without corporal punishment it was impossible to maintain the discipline and efficiency of the army. Singular that these men were so prejudiced that they could not be made to understand, that soldiers could be treated and ruled like rational beings.

"I am not the least surprised at this opinion," says Lord W. Bentinck. "I must not forget that for many years of my life, in conjunction with 99 hundred part of the officers of the British army, I entertained the same sentiments. It is only from long reflection, from the effects of discussion, from the observation, that since that time, though corporal punishments have diminished an hundred, perhaps a thousand fold, discipline has been improved, and the soldier treated as a rational being and not as a mere brute, that my own prejudices and that of others have given way. I now feel confident, that the degradation will speedily disappear before a reasonable and enlightened legislature, even in the British army."

Attempts had been made also more than once to ascertain to what extent it was legal to inflict second punishments, that is, bringing out soldiers who had undergone so much of the sentence awarded as they could, to receive the residue in subsequent instalments. At length an opinion was obtained from Mr Manners Sutton, then judge-advocate, in the course of debate in the House of Commons on the 21st June 1815, that "when a criminal had been brought out and had suffered some portion of the lashes to which he was sentenced, it was illegal to inflict any more on him at any future time, or, by the threat of inflicting them, to compel him to enter into any other regiment."

This was long after the cruelty and injustice of this mode of inflicting punishment had been denounced by various respectable and competent authorities, Dr Hamilton, Sir Robert Wilson, Sir Samuel Romilly, Sir Charles Napier, and many more. By this illegal practice, indeed, many a soldier had been killed; and many had been lost to the service, with excessive loss and waste of the public funds, independent of its inhumanity. Late, however, as this point was established, its establishment was of great moment to the soldier. Other amendments and ameliorations followed, chiefly in consequence of the general feeling of abhorrence and disgust at the practice, and also, it may be added, in consequence of the frequent discussion of the question in Parliament. At length, on 22d June 1834, after various motions had been thrown out, one for the returns of corporeal punishment in the British army, for a period of five years, was agreed to by the secretary at war; and from the obstinacy with which this had been previously refused, it is impossible to doubt, that the granting the return was to be a prelude to some considerable change. At length after the subject had been repeatedly agitated in the House of Commons in 1836, it was ordered, we presume, from the Horse Guards, that the award of a general court-martial was limited by the articles of war to 200 lashes, of a district court-martial to 150, and a regimental court-martial to 100. This proceeding leaves the evil in all its degrading and debasing effects, but it takes away the greater part of its cruelty and torture, and what is of still greater moment, the frequency of inflictions of this kind became greatly limited.

The following Table is given by Mr Marshall to show the rates at which court-martials have been held, and punishments have been inflicted in the British army between 1824 and 1835, a space of eleven years.

Years.	Establishment.	No. tried by court-martial.	Propor. of men tried to Estab.	No. sentenced to punishts. other than corporal.	No. on whom corporal punish. was inflicted.	Ratio of corporal punishments per 1000.	Propor. of men who received corp. pun. to Estab.
1825,	98,948	4,708	1 in 21	2,280	1,737	17	1 in 59
1826,	111,058	5,524	1 in 20	2,653	2,242	20	1 in 50
1827,	111,107	5.340	1 in 21	2,541	2,291	20	1 in 50
1828,	110,918	5,314	1 in 21	2,779	2,143	19	1 in 52
1829,	103,749	4,789	1 in 22	2,705	1,748	17	1 in 59
1830,	103,374	5,946	1 in 17	3,847	1,754	17	1 in 59
1831,	103,413	7,438	1 in 14	5,549	1.889	14	1 in 71
1832,	103,572	8,780	1 in 12	7,215	1,283	12	1 in 83
1833,	103,527	9,628	1 in 11	8,320	1,007	9	1 in 111
1834,	103,063	10,212	1 in 10	8,946	963	9	1 in 111
Total,	1,052,729	67,672	1 in 17	46,835	16,957	15·4	1 in 70·5

"This statement," says Mr Marshall, "shows that the mean number of soldiers who were annually accused and tried during the period comprehended in the return was 1 in 17,—the lowest ratio being 1 in 22, and the highest 1 in 10. The ratio of corporal punishment has decreased by one-half. It is, however, obvious that while the infliction of flogging has been reduced, the number of alleged military delinquencies has been increased. How far the latter may be a consequence of the former, I am unable to decide. Not only are the number of corporal inflictions reduced from 20 per 1000 per annum to 9 in 10 years, but if the limited sentences be taken into account, perhaps the number of lashes inflicted in 1834 did not amount to one-fourth of the number inflicted in 1825. Sir Henry Hardinge stated in evidence before the Commissioners on Military Punishments in 1836, 'That, including men who have been serving for upwards of 20 years, the average of punished men throughout the army does not exceed 36 men at one time in regiments of 700, or three men and a half a company,' being about 5 per cent. Consequently, the unpunished men are twenty times more numerous than the flogged men. This is a highly gratifying account of the result of popular feeling being directed to ameliorate an alleged grievance by means of public discussion. It indicates a great improvement in the condition of soldiers, without injury to the discipline and efficiency of the army. How different is the present state of affairs in this respect from the time when, as is confidently alleged, the punished or degraded class of the army exceeded in number the unpunished."

The army is further represented to be in the most efficient state. From this it evidently results, that terror, coercion, torture, and punishment can not only be of no use in maintaining the efficiency and discipline of the army, but that these means are actually destructive of discipline, by the ignominious degradation which they produce, and the reckless and desperate characters which they have uniformly made.

Mr Marshall joins with all persons of correct views in disapproving of the false and unjust, and not unfrequently dangerous, position in which medical officers were placed by the system of corporal punishment practised in the army; and he gives exmples, in which the medical officer had, apparently without any blame on his part, been made the agent of a fatal punishment, and have fallen into discredit, or been superseded, in consequence. This is indeed only one of the effects of a great system of cruel and unbecoming torture; and, were it for no other reason than making the medical officers accessary to the evils of corporal punishment, it would be well to get rid of the practice altogether. The medical officer, by being made to measure, as it were, the amount of punishment, is placed in the eyes of the soldier as a sort of assistant executioner; and, consequently, instead of being looked on as a patron and protector, he is very likely to be considered as a scourge and an enemy.

The great practical inference, however, to be deduced from the whole of this enquiry is, that there must be certainly means of governing soldiers, without having recourse to the barbarous, disgraceful, and degrading system of corporal punishment. This Mr Marshall considers to be as nearly as possible demonstrated, not only by referring to the experience of intelligent military officers, but by comparing the state of those regiments, in which punishments are rare, with those in which they are more frequent. Mr Marshall also very justly attaches great importance to the circumstance that military service has been, and is still, a sort of state, unnatural, constrained, and discouraging to the great mass of the population who would otherwise enter it. The service should, he thinks, be limited to a certain time; the inducements to enter it ought to be stronger; the rewards ought to be greater, more liberal, and such as would induce a set of well-behaved men to join the service; and when the character of the army was by these means raised, irregularity, vice, and crime would be in a great degree put out of countenance.

To education also Mr Marshall assigns great influence, in improving and elevating the moral character of military bodies, and consequently rendering the men better soldiers, and better servants of the public. It must be gratifying to every one who has sincerely at heart the welfare and prosperity of the army to see, that Mr Marshall does not participate in the vulgar error, that bad men make good soldiers; and that any one who is unfit for the steady duties of civil life will execute the duties of the soldier well. Never was a more absurd and wicked sentiment uttered or acted on; and the instances, which have been supposed to confirm it, are manifestly exceptions to the general rule.

To one vice, doubtless, the soldier has been too prone; and to that one almost all his other errors and misfortunes may be traced. Drinking spirituous liquors to excess, and its pernicious effects, have been absurdly supposed to be the privilege of the soldier, and a sort of reward for his toils. Yet by whom has the soldier in all this been encouraged? Do not the officers often set the example; and does not the government itself, by establishing canteens at home, and giving spirit rations abroad, incur the weighty and not guiltless responsibility of making him believe that he was doing nothing wrong, if they did not tell him in pretty plain terms that he was in the right, and doing something very meritorious? When the soldiers see and know, that the officers of the regiment sit for several hours in the evening, taking quantities of intoxicating liquors not very moderate; when they see this repeated daily; and when they know, as they must often do, that it is with many carried to the very verge of insensibility, what are they to think? Certainly, that if the thing is not quite right and virtuous, yet it is so far correct, because it is done by their superiors. The difference they believe is only in degree, and in the kind of liquor drank.

It is, however, fortunate that this practice is on the decline; and, were it altogether abolished, it would be the happiest day the army ever witnessed.

Many of the evil habits of mankind are caused by imitation, and by want of the power of reflecting on the tendency and effects of these habits. So is it with the habits of soldiers; and above all with those which have been believed to require the coercive and degrading measure of corporal punishment. Excluding always artificial delinquencies and offences, that is, those which are made by the habits and fancies of military life, many of the errors might certainly be corrected by the same means by which they are corrected in civil life. A man sees in others the bad effects of drunkenness and irregularity, or rudeness, dishonesty and immorality, and these excesses, improprieties, and iniquities he accordingly avoids; or he has felt in his own personal experience the bad effects of deviations from the right line of conduct; and these he accordingly abandons, and determines on a different line of proceeding. This is the moral check, and its effect, in inducing reformation and improvement. But among soldiers it appears as if one lapse, one deviation, was to be permanent in its effects, never to be recovered from, never to be forgiven. Their officers seem to presume that if a man has been once drunk, he must be a drunkard, and treated as a drunkard all his life. If he has been once behind the time at tattoo, he must be forever regarded as an irregular character. If he has once appeared with linen not quite clean, he is to be regarded as always a dirty, slovenly person. Surely if soldiers are men, they might be taught to eschew what is wrong, and follow what is right, by the same means by which other men are led to amend their errors.

All of us are the creatures of circumstances, much more so than we imagine or are willing to admit. Soldiers are merely what they are made by circumstances, situation, and opportunities. Have any measures been devised or adopted to improve them; to give them more virtuous, regular, and sober habits; and to show them that from drunkenness and debauchery, idleness and evil habits, cock-fighting, duck-hunting, and gambling, no good can result; and that in other useful and innocent occupations there is more real enjoyment, more actual pleasure? If to these questions any answer can be given in the affirmative, that answer is applicable to very few regiments indeed.

One word more and we finish. When a regiment either of infantry or of cavalry, much more the latter, is beheld on march or on the review ground, in all the pomp and circumstance of military magnificence, who would imagine that the man who performs his evolutions with such precision and energy,—that the soldier who manages his horse as if horse and man were one being, may be, nay has been, bound, stripped, and scourged, in a manner in which we should be ashamed to scourge a rated hound. How is it possible to respect that man? How can he respect himself? The day that sees a man a slave, says Homer, shows that he has lost half his worth; the day that beholds a man scourged, it may be said, shows that he has no longer any worth. Surely the Romans were a wise and sagacious people, who would not permit the Roman citizen to be scourged or beaten with stripes; because they saw, that, from that day forward, the man so disgraced must be no longer worthy of the rights and privileges of citizenship. This noble and exalted feeling it was which made the Roman soldier a being so superior, which made him feel that his destiny was,—

Regere imperio populos,
pacisque imponere morem,
Parcere subjectis, et debellare superbos.

Surely it would be well if legislators and other persons in authority, would take this matter into serious consideration, and determine to treat the soldier, as far as possible, as a citizen,—as a man having rights and privileges to enjoy, as well as duties to perform.

In attempting and recommending improvement in the condition of soldiers no one has laboured more assiduously than the author of the present tracts. After spending the greater part of an active life in the medical service of the military force, he has applied the manifold and correct experience he has acquired in making known the sufferings and the real merits of the soldier. Mr Marshall has shewn how often he has been wantonly and unjustly oppressed; how frequently his soul has been made to enter into the iron of petty tyranny; he has shewn how much real evil this system causes in the service, and how much better the object might be attained by mild treatment, reasonable behaviour, and general kindness.

ART. V.—*A Practical Treatise on Inflammation, Ulceration, and Induration of the Neck of the Uterus; with remarks on the value of Leucorrhœa and Prolapsus Uteri as Symptoms of Uterine Disease.* By JAMES HENRY BENNETT, M. D., Licentiate of the Royal College of Physicians; formerly House Physician to the Hospitals, St Louis, Notre Dame de la Pitié, and La Salpetriere, Paris, &c. &c. London, 1845. Post 8vo. Pp. 212.

OF a considerable proportion of diseases the presence is indicated by two orders of symptoms; the one the rational, or the dynamic and sensational symptoms, or those which indicate their presence in the functions and feelings of the patient; the other the critical, physical, or sensible signs, or those which may and must often shew themselves by certain sensible signs, and which may be ascertained by examination, either manual, ocular, auscultatory, or instrumental. Though at different periods attempts have been made to employ the one of these orders of symptoms only, to the exclusion of the other, yet it cannot be doubted that they ought at all times, and in all cases, be taken conjointly. The rational and dynamic signs cannot be always understood without knowing the critical and physical signs; nor are the latter alone of much value without giving attention to the rational signs. The rational and dynamic signs of diseases are often of a general or constitutional nature, affecting the whole frame and system. The critical and physical signs are commonly local, and manifest their existence by certain changes, physical, physiological, or pathological, in the textures and organs affected.

These observations are particularly applicable, among other diseases, to those of the female organs of generation. Many of these, very different in nature, give rise to much the same sort and degree of rational and constitutional signs. But all these differ more or less in physical symptoms; and hence, without some knowledge of the physical signs, all treatment of these disorders is empirical and groping in the dark.

Obvious difficulties have stood in the way of much attention being paid to the physical signs of these diseases in this country. The habits and feelings of society, and the natural delicacy of the sex, have probably prevented physicians from observing the symptoms of this order so easily, and obtaining on them information so correct as they could on the rational signs; and though part of this difficulty may subside in process of time, it is possible that it may never be entirely removed.

Abroad, and especially in France, it is very different. There neither physicians encounter, nor do females present, great difficulties in the way of examining the organs of generation; and, consequently, the use of the speculum for inspection, and manual examination for ascertaining the exact state of the vagina, and the neck and orifice of the womb, and every means of procuring information on the condition of the parts, are in constant and habitual employment. The facilities accordingly for studying this order of diseases, for observing their progress and effects, and conducting their treatment, are much more multiplied, and much more accessible in Paris especially, than in any city in this country.

There can be no doubt that, in many instances, for the successful and efficient management of these diseases, examination and inspection are indispensably requisite; and, of the treatment recommended and enjoined, without this preliminary information, much must be empirical, desultory, and inappropriate.

Inflammation of the neck of the womb, the author represents, with its effects, ulceration and induration, to be a very common affection; more so, he says, than English practitioners are aware. That is, the disease is very common in France, and extremely common in Paris. In this country it does in a certain degree and form prevail, but evidently not to the same extent as in France. It is nevertheless impossible to doubt that the loose and immoral habits of French society have rendered this order of diseases more frequent and more prevalent than in many other countries. With this, however, we in our present vocation have no concern. There the diseases are, and require to be removed, healed, or alleviated. How they came there is important to the pathological etiologist, as well as to the moralist. But it would be wrong to refuse relief to the diseases of the sex, from the errors and failings of a portion of them; nor would it be the character of the prompt physician to withhold the means of relief until he had determined the cause of the disease.

The author regards every form of *prolapsus uteri* and leucorrhœa as dependent on some degree of this affection; and there is no doubt that he is in the right to a certain extent. He asserts also that he has ascertained to his complete satisfaction three points; *first*, that excluding cancerous disease in the majority of adult females who have been exposed to sexual intercourse, a confirmed leucorrhœal discharge, whatever may be its nature, is accompanied by inflammation of the neck of the womb; *secondly*, that this inflammation seldom exists long without producing ulceration; and *thirdly*, that ulceration is always accompanied with more or less swelling, with or without hardness of the substance of the uterine neck.

He distinguishes, therefore, between the inflammations and ulcerations which take place in the uterine neck of females who have never conceived, and those which take place in the same region in females who have conceived, that is, who have either miscarried or borne children. Besides, there are to be noticed syphilitic ulcerations, which may take place in both classes of women, and malignant or cancerous ulcerations.

It is allowed that the *cervix* and *os uteri* contain a little cellular tissue, of which the body of the uterus is devoid; that it is much more vascular than other parts of the organ, and that it presents few muscular fibres. Dr Bennett, with M. Jobert, admit the existence of circular muscular and longitudinal muscular fibres.

It is probably as important to advert to the mucous structure of the orifice and neck of the uterus. The mucous membrane is thin, but vascular, and liberally supplied with muciparous follicles.

The size and length of the cervix vary in different females, from a few lines, to one inch and a half or more.

In the unimpregnated state, and when not morbidly enlarged, the body of the uterus moves readily on pressure being made on the neck; pressure thus applied, acting on one extremity of a lever, that is, the author says, raising the other in the opposite direction.

In virgin females the author has not observed sufficiently to speak confidently as to inflammation attacking the neck and orifice of the womb.

In married females, however, any slight irritation of the internal organs is certain to cause morbid action in the *cervix uteri*, which is bruised and reddened, the irritation proceeding to inflammation and ulceration, little disposed to heal. In many instances sexual intercourse alone may give rise to this state. The orifice of the uterus in part, or altogether, becomes swelled, prominent, red and hot; secretes then a muco-purulent discharge; and though pain be not felt in the parts, it is felt in the loins; and pain is also felt upon pressure or examination, or on occasion of intercourse. Pain is also felt in the groins, in the hypogastric region, and behind the *os pubis;* and the patient often feels sore and as it were bruised. The pain in the back and loins is particularly distressing. It is gnawing, and though seldom entirely gone, it admits of periods of alleviation and aggravation.

When these complaints are made, it is requisite to examine manually, and to employ the aid of the speculum. By the first means it is ascertained, that the orifice of the womb is large, prominent, tumid, and generally painful on pressure; the vagina at its upper part is hotter than usual; the surface of the uterus is less unctuous or moist than usual; and the surface is smooth, and though tumid, it is unresisting. By specular inspection it is found that a quantity of muco-purulent matter covers the upper part of the vagina and *os uteri* from where it proceeds. The neck of the womb is enlarged. The swelling is generally most remarkable at the upper or anterior lip, and the orifice and neck are red and prominent, while the mucous surface may present white or red vesicular elevations, the result of distension of the mucous follicles. Sometimes the membrane is observed, when the mucus is removed, to present abrasions, excoriations, or ulcerations. The orifice of the uterus is generally irregularly closed. Menstruation is more painful than in health, and leucorrhœa in greater or less amount takes place in the intervals.

This is the usual aspect of the disease in the unimpregnated female. The uterine neck is not deeply but superficially affected. Though swelled and congested it is soft, at least not indurated; and this is in general the great difference between the disease in the unimpregnated, and in females who have had several children.

So long as this disordered state of the uterus continues, it renders impregnation impracticable. The female remains barren. But when the disease is removed. which it may be under remedies or spontaneously, then impregnation takes place.

This affection the author thinks the same as that described by English practitioners under the name of irritable uterus. It is cured by the use of cauterization by the acid nitrate of mercury, and subsequently astringent injections. We have seen it disappear under abstinence from intercourse, rest, and injections at first tepid, then cold.

In the womb which has been once or several times the seat of the congestion and distension consequent on impregnation, the disease is more formidable, and generally more obstinate. The affection, indeed, seems to be the result either of labour and its effects, or abortion and its consequences; and in one only of twenty females, the author thinks, will this severe form of the disease be found to take place in a person who had never conceived.

Dr Bennett puzzles himself and his readers to explain how the muccus membrane of the uterine cavity becomes dissociated, as he terms it, from the subjacent tissue, while that of the uterine neck becomes more perfect as pregnancy advances. He thinks, that as the neck and orifice are rapidly dilated towards the close of pregnancy, this process must in many, if not in all cases, be accompanied by erosion, laceration, and contusion of the membrane. Though in ordinary circumstances these may be speedily healed, he thinks from what he has observed, that there occurs cases in which they do not enter into this reparative process. Is it not as likely that these evils do not take place in all cases, and that they occur only when the parts are exposed to bad management, or are previously in an unhealthy state?

The symptoms furnished by digital examination and specular inspection are much the same as in the first form. There is, however, more frequently induration of a part or the whole of the *cervix uteri ;* and, even though this may not be manifest at first, it takes place in the subsequent course of the disease. The *cervix* becomes thick, massive, and congested, in other words hypertrophied; and the gravitating weight of the neck causes more or less *prolapsus uteri.* The *cervix* is brought nearer to the *vulva;* at the same time it may fall backwards and press on the posterior portions of the *vagina*, while the body of the uterus is carried more or less forwards, forming anteversion of the uterus, or retroversion of the neck. In short, an unusually large quantity of blood is thrown into the vessels of the neck and orifice of the womb; fibrin and lymph are effused into the interstitial substance; and by these means the neck and orifice are hypertrophied, or enlarged and indurated. The vagina is also relaxed, and this contributes, the author argues, to the prolapsus which takes place. This abnormal laxity of the vagina may be the effect of the disease itself; the distension of the superior portion of the vagina diminishing its tense or contractile power. There is also, from increased afflux of blood to all the parts, and consequent leucorrhœa, much relaxation of the vagina.

This disease always gives rise to severe symptoms. The sense of weight and heaviness in the hypogastric region becomes marked and distressing, especially in standing and walking, and there is often a sensation as if something were falling down out of the vagina entirely. The pains in the loins and back are also severe and urgent, and pains are felt in the groins, hips, and thighs, and along the course of the different nerves.

The health at length gives way. The patient is feeble, dyspeptic, nervous, unable to do or endure much, and becomes emaciated. Menstruation is generally disordered, being diminished in quantity and shortened in duration, but accompanied with much more distress and suffering than usual. It appears to aggravate all the symptoms of the disease.

By examination, the increased size of the cervix and the prolapsus are at once ascertained. The *os uteri* is more or less open, so as to admit a portion of the finger; and the velvety sensation of the ulcerated surface is evident, while the granulations are fungous or prominent.

The cervix may be deeply fissured, so as to present several lobes or lobules; changes which the author ascribes to difficult or instrumental labour, but which we are inclined to regard as the effect of the unusual swelling of the neck and orifice. This is not necessarily attended with the formation of ulcers. Yet ulceration in patches does occur in certain cases.

The author, we think, from following too much the distinctions of the French physicians, has not given of them a very clear or distinct account. He informs his readers, that tubercular ulceration is the ulceration which follows the softening and rupture of tubercles in the uterine cervix.

In examination with the speculum, the ulcerated surface is found covered with large, livid, fungous granulations, easily bleeding. The truth is, that these ulcers may proceed from two sources. They may either be the ordinary follicles of the uterine neck in a state of ulceration, or they may be the result of indurated and swelled portions of the substance of the uterine neck.

This form of disease is to be treated by rest in the horizontal position, the local application of nitrate of silver, or acid nitrate of mercury, baths, aluminous injections, and light unstimulating diet.

Various other ulcers and ulcerations are observed at the orifice or neck of the uterus, and as these take place chiefly in females of loose or absolutely immoral habits, the question comes to be whether they are syphilitic. Dr Bennett infers, from a great extent of observation, that the genuine chancre, such as it is described by English surgeons, with its usual physical characters, is very rare in the *cervix uteri.* He allows, however, that in patients labouring under blennorrhagia, or primary, secondary, or tertiary syphilis, ulcerations presenting the character of inflammatory ulceration are very common, that is, we understand, that inflammation attacks the *cervix* and proceeds to ulceration.

On cancerous ulceration we need not dwell. The disease presents the usual characters, and runs its usual course.

This small performance contains a large amount of useful information. The most useful part, however, is the therapeutic, or that on treatment. The value of the diagnostic portion also we willingly admit; but the general opinion will be, that the therapeutic is that in which the information of the author appears to greatest advantage.

The great remedy is cauterization of the parts by nitrate of silver, acid nitrate of mercury, or pure potass, *potassa fusa*, as he calls it, applied to the surface by suitable means, and after proper intervals. Next to this must be ranked the use of astringent injections, sulphate of zinc, alum, or acetate of lead.

We are pleased to see also that he recommends the use of depletion by means of leeches to the *os uteri*, from which we have observed much benefit proceed; and we are no less pleased to see, that the censures the use of pessaries and similar mechanical agents.

Absolute rest and moderate diet with regulation of the bowels, must not be omitted.

In conclusion, the book deserves the attention of all who wish to understand the nature of the diseases considered.

Art. VI.—*Lectures illustrative of various Subjects on Pathology and Surgery.* By Benjamin C. Brodie, Bart. F. R. S. Serjeant Surgeon to the King, &c. &c. London, 1846. 8vo. pp. 411.

The contents of this volume have been already for a considerable time before the public. We therefore do not propose to give any review of them. Neither is it necessary; because while they are doubtless valuable and instructive, proceeding as they do from one of the most experienced and skilful surgeons of the day, the materials do not possess the character of novelty. Several of the subjects, indeed, have been more or less fully considered in the pages of this Journal, at very recent intervals. This is particularly the case with the subject of mortification, to which, in the present volume, seven chapters are devoted.

It is nevertheless impossible for Sir Benjamin Brodie to speak of any subject in surgery, either pathological or therapeutic, without communicating useful information. Of this in the present volume the reader finds a large proportion; and what is of not less moment, he will find it delivered in an easy manner, and with much judgment and common sense.

One observation has forcibly struck us on the perusal of these lectures; and this we mention as another proof of the sense of the author. It might be inferred, that as the lectures were avowedly and professedly on surgery, the author would be frequently, if not constantly, recommending operations and the use of instruments. His procedure is entirely the reverse. He in general concludes by censuring, sometimes condemning operation entirely, and trusts much for favourable results to patience, time, and judicious medical management. If the student read this book aright, in short, he will be disposed to think that operations are very often useless, and in a large proportion of cases extremely hurtful.

This book, indeed, is the most severe blow that operative surgery has received for the last fifty years. It proves what has long been our opinion, that the knife is in many cases nothing, and that medical treatment is every thing. Let the practical directions in these lectures be contrasted with the cuttings, the sawings, the cauterizings, and all the other manual performances of surgery, inculcated about fifty years ago; and the difference is immense. One question only may arise. Is this change in the art, or in the author, or in both together? We think that we must consider the change in the art itself; and we are not without hopes, that at some golden period of surgical science, all dismembering and mutilating operations will be abandoned; and that the surgeon, even the most pure, will become a medical surgeon, as he actually is, and operate only when he is to open an abscess or perform venesection.

PART III.

MEDICAL INTELLIGENCE.

ANATOMY AND PHYSIOLOGY

Spermatozoa of the Bear.—There is scarcely a physiological question of more interest than that of the true nature of the spermatozoa. Whether they be independent animalcules, or only free and floating cilia, has never been clearly proved, although Professor Valentin described an amount of organization in the spermatozoa of the bear, quite sufficient, if true, to show that they really are independent beings. His observations are given in the English version, by Dr Willis, of Professor Wagner's Physiology, p. 228.

Mr Gulliver, availing himself of the opportunity afforded by the death, from acute disease, of the magnificent Polar bear in the menagerie of the Zoological Society of London, examined the spermatozoa of that animal, and communicated his observations on the subject to the meeting of that society, February 24, 1846.

The spermatozoa were very abundant in all stages of development in the seminal tubes, and especially perfect in the vas deferens. Mr Gulliver could perceive no marks of mouth, anus, and internal vesicles, such as Valentin has depicted in these spermatozoa, but found them in all respects similar to those of several other mammalia, as was shown by a comparison with Mr Gulliver's preparations or drawings from the stoat, Indian badger, dromedary, camel, red deer, fallow deer, and wapiti.

On the size of the Red Blood Corpuscles of Birds. In communicating to the meeting of the Zoological Society of London, March 24, 1846, some measurements lately made by Dr Davy, of the blood corpuscles of a humming bird, Mr Gulliver remarked that they are the smallest yet described among birds. Referring to his collected measurements of the blood corpuscles of vertebrata, published in the Proceedings of the Zoological Society, October 14, 1845, and noticed in the last number of the Edinburgh Medical and Surgical Journal, Mr Gulliver observed, that, although there is no relation between the size of the blood corpuscle and the size of the animal, in mammalia of different orders, there is a relation of this kind in the same natural family, as in the ruminants and rodents,—*i. e.*, the larger species of one family have regularly larger corpuscles than the smallest animals of the same family. Now, he shows that the law as to the size of the blood corpuscles is the same in the entire class of birds as in a single family of mammals; so that his copious tables of measurements contain no example of large blood corpuscles in the smallest birds, or of comparatively minute corpuscles in the largest birds.

On the Cerebral Organ of Speech. By M. Belhomme. (*Bulletin de l'Academie Royale de Medecine*, April 1845.)—M. Belhomme read a memoir entitled, "on the Localization of Speech in the anterior lobes of the Brain," in which, by a reference to ten cases which he narrated, he endeavoured to prove that the cerebral organ which regulates speech was seated in the anterior lobes of the brain. From the facts and reasonings brought forward in his paper, M. Belhomme arrived at the following conclusions.

1. Affection of the faculty of speech depends either on a cerebral affection, or on a lesion of the organs of communication between the brain and the organs of speech.

2. The sudden loss of speech depends on a hemorrhagic or other lesion of one or more frequently of both, anterior lobes of the brain.

3. It is necessary to guard against confounding convulsive and paralytic disorders which affect the power of speech, with that sudden loss of memory of words, and consequently difficulty of speech depending on affection of the anterior lobes of the brain.

4. In disorder or partial destruction of the anterior lobes of the brain, the speech is suddenly arrested, and it is only after a cicatrix has formed in the brain that the organ recovers more or less of its former function.

New Researches on the Movements of the Heart. By Signor D. Gola. (*Annali Universali de Medicina*, April 1845.)—Most physiologists are agreed that the impulse of the apex of the heart against the thoracic parietes correspond with its systole. The contrary opinion has, however, been maintained by several eminent writers both in this country and in France. Signor Gola's researches were undertaken with the view of ascertaining to what movement of the heart the impulse of the apex against the thoracic parietes corresponded. Having with much care exposed the heart of a frog, he found that the contractions of the heart were accomplished in the following order.

The systole of the auricle preceded the diastole of the ventricle; in its dilatation the ventricle increased at its base by three lines in diameter. In proportion as the ventricle became distended, the surface became flattened on its greatest diameter, and the whole heart executed a slight movement of rotation from left to right, in consequence of the dilation being greatest towards the right side of its base. By virtue of this rotatory movement, the point moved about a line towards the left side, but was not sensibly elevated.

To the diastole succeeded the systole of the ventricle, and during it the transverse diameter of the ventricle at its base diminished by three lines, but this part of the heart gained in height what its lost in breadth and in extent; it took a lengthened form, and the apex was elevated from its plane and sensibly carried forwards.

When a fine straw, fixed at its two extremities, was laid across the heart, so that during the diastole it simply touched the surface, it was sensibly raised during the svstole. By placing this straw longitudinally to the axis of the heart, and allowing one end to move over a graduated scale, he found that while during the diastole the straw was only raised to the extent of one-tenth part of a centimeter, during the systole it was raised eleven and one-half tenth parts of a centimeter.

These and other experiments led Signor Gola to conclude that the impulse of the apex against the ribs and the contraction of the auricles coincided. In conclusion he remarks, that he has often introduced his hand into the pericardium of animals while being slaughtered for food, and invariably remarked that the heart during its contraction struck with force against the hand, and that the impulse was sometimes so strong as to cause a disagreeable sensation.

On the use of the Cerebro-Spinal Fluid. By M. LONGET. (*Comptes Rendus des Seances de l'Academie Royale des Sciences,* June 1845.)—The common opinion of physiologists for many years has been that the removal of the cerebro-spinal fluid very materially interferes with the powers of locomotion. The common mode in which the operation for this purpose has been performed was by opening the *dura mater* and arachnoid membranes between the occiput and atlas, after having cut through the parts which cover the occipito-atlandoid space. When this operation is performed, the animal walks with a staggering gait, as if under the influence of alcohol.

M. Longet, in order to ascertain whether any of the effects observed depended on the incision of the soft parts, cut through these parts alone in the horse, dog, cat, guinea pig, and rabbit, without incising the *dura mater,* or giving issue to the cerebro-spinal fluid. When this was done and the animals were allowed to rise, he was not a little surprised to find that all the effects said to be caused by evacuation of the cerebro-spinal fluid had been produced—that the animals staggered as if under the influence of alcohol. He therefore endeavoured to draw off the fluid without interfering with the parts at the nape of the neck, in order to ascertain what effects were produced by such a loss. For this purpose he removed the lamina of one of the dorsal vertebræ, about the middle of the dorsal region. In consequence of this operation marked feebleness of the hinder extremities was produced; but this weakness was not increased by removal of the cerebro-spinal fluid, and, moreover, the animals did not present that symptom of staggering so remarkable in those in which the soft parts of the nape of the neck were interfered with.

M. Longet therefore concludes that preceding experimentalists were wrong in attributing to the cerebro-spinal fluid an important influence over the locomotive function; and that the staggering walk of animals operated on by section at the nape of the neck is to be attributed entirely to the effects of cutting through the parts there, and not to the removal of the cerebro-spinal fluid.

New Researches on the Production of Fat. By M. BOUSSINGAULT. (*Comptes Rendus des Seances de l'Academie Royale des Sciences,* June 1845.)—During the course of the year 1844 MM. Boussingault, Dumas, and Payen endeavoured to prove by a reference to experiment that the whole fat of animals was derived from the nutriment on which they fed. This opinion was vigorously opposed by several, and to show its fallacy MM. Dumas and Milne-Edwards repeated the experiment of Huber on the formation of wax by bees when fed solely on sugar. The results arrived at by these experimentalists showed that Boussingault's theory required modification, as it viewed the subject in one light only. M. Boussingault, accordingly, repeated his experiments in different ways, and now very considerably modifies his former opinion; having now ascertained that the presence of a small quantity of fat in the food causes the production of a much larger quantity than is naturally contained in the food consumed. The following are his conclusions:—

1. Pigs eight months old after having been fattened contain much more fat than they have taken with their food.

2. Pigs fed for six months on potatoes yield no more fat than what was contained in the roots they consumed.

3. In fattening pigs, as the result of nine experiments, there was more fat produced than was contained in the food on which they were fed.

4. Aliments which, when given singly, have not the property of fattening, acquire this property to an astonishing degree if mixed with fatty matters, although fat given alone produces inanition.

5. Fattening articles of food which do not contain much fat are always

rich in azotized matters. In fattening geese the fat produced always exceeded the quantity contained in the maize on which they were fed.

The mode in which M. Boussingault ascertained the astonishing power which fat mixed with the food produced in fattening an animal, was by experimenting on ducks. Some ducks were fed on rice alone, a substance which contains only a few parts of fat in the thousand. Others of the same brood were fed on rice with an ascertained quantity of butter added. At the end of the experiment the first were as lean as when first put on the diet, the latter in a few days became positively balls of fat. In all his experiments M. Boussingault constantly observed that muscle was formed along with fat in all fattened animals.

PATHOLOGY.

Case of Tubercle on the Tuber Annulare. By M. MAHOT. (*Archives Generales de Medecine,* July 1845.)—A soldier, 22 years of age, of delicate constitution, but enjoying good health, was found in an insensible state on the night of the 8th of January. He was foaming at the mouth, and the left arm and leg were rigidly contracted. Cold water dashed on his face restored his intelligence, but a fit of the same nature recurred in about four hours. For ten days after this he had extreme weakness of the left arm and leg, with imperfect power of motion. When in the hospital he was seized with measles, and on recovery from this it was observed that he was affected with paralysis of the left side of the face and of the left arm and leg. The paralyzed parts retained their sensibility. The intellectual faculties were entire; the eyes were fixed; he lay prostrate; and the pulse was from 50 to 55 per minute. Two days after this, 23d of March, strabismus came on. On the 24th, he had a fit of insensibility, during which the respiration was stertorous, and he was foaming at the mouth. On the 26th he could not turn the right eye outwards. From this day the headach increased, and he was annoyed with repeated attacks of vomiting and convulsions till the 17th of April, when he died.

The membranes of the brain were somewhat thickened. The ventricles contained much turbid serous fluid, and the walls of the ventricles were softened and broken down. The internal arachnoid membrane of the ventricles contained a cyst of the size of a cherry stone filled with tubercular matter, in the centre of which was contained a cretaceous concretion. The *tuber annulare* had an irregular form, caused by a sensible elevation on the right half of its lower surface, which was much harder than natural. On incising this body, there was met with a tumour about the size of a chestnut, occupying the right side of the tuber, but approaching close to the mesial line. It was of a firm consistence, without softening, of an ashy gray colour, and quite distinct from the cerebral substance, which was a little softer than natural.

It is remarked as the most curious point in this case, that the pressure of the tumour on the origin of the external motor nerve of the right eye had been the probable cause of the inability to turn that eye outwards. The paralytic symptoms were also on the opposite side of the body from that of the affected portion of brain.

New Researches on the Œdema of new-born Infants. By M. HENRY ROGER. (*Archives Generales de Medecine,* May 1845.)—In an interesting paper on scleroma or the œdematous hardening of the cellular tissue of new-born children, M. Roger points out the important fact, that in proportion to the extent and severity of that disease, is the lowering of the temperature of the child. This lowering of the temperature is not confined to the affected parts, but pervades the whole system, and seems to be essentially connected with the pathological states which cause the disease. M. Roger gives a table of the temperatures of 29 cases of this disease, in all of which it was observed that the more severe the affection the lower was the temperature. Thus in the cases in which the œdema was slight the temperature was normal, *i. e.* 98° Fahrenheit, or one or two degrees below it; but in the severe cases, even at the outset of the disease, the temperature was found so low as 75°. This was the lowest temperature noticed in any of the fatal cases; and it is remarkable that it is the same as that which was found to prove fatal to animals which were starved in the experiments of Chossat on inanition.

In commenting on the other pathological appearances met with in the bodies of these infants, M. Roger directs especial attention to those of the lungs which are in their nature identical with those observed following the pneumonia of infants. He therefore considers that authors are much mistaken in calling that disease inflammatory, or those changes products of the inflammatory action which are only observed in infants whose temperature is lower than natural, and whose whole functions indicate torpor and congestion, but not inflammation.

Ulceration of the Gums in Children occurring in an Epidemic form. By James Duncan, M. B. (*Dublin Journal of Medical Sciences,* September 1845.)—The disease described by Dr Duncan is a severe and fatal ulceration of the gums and mouth of young children, attended with high fever, and apparently of an epidemic origin. Diarrhœa was the first symptom of illness observed, but there was no unusual pain of the bowels. The evacuations were unhealthy, watery, but not generally deficient in bile. In some cases, however, they were whitish, and very offensive. Blood was discharged in almost every case, either in a fluid state or mixed with jelly-like mucus. After these symptoms had continued a week or ten days the soreness of the mouth attracted attention. The gums were then found ulcerated, the fangs of the teeth exposed, and covered with a yellowish-white sordes. As the disease advanced the gums lost their pale flesh colour, and became red, swollen, and spongy, and bled both spontaneously and on being touched. The breath gradually became offensive, and the secretion of saliva became excessive. Partly from the fever, partly from the diseased state of the gums, the children could not take food, but their thirst was often excessive. The fatal termination seemed rather due to the violence of the attending fever, or the severity of the diarrhœa, than to the diseased state of the mouth. Though the diseased condition of the mouth closely resembled that produced from the administration of mercury, this drug excited rather a beneficial influence when exhibited, and tended to modify the disease.

In commenting on the nature of the affection, Dr Duncan endeavours to show that the impaired state of the constitution is its true cause, and that in many points it presents an analogy to *purpura hæmorrhagica.* One very interesting case is related of one of the members of a family being attacked with ulceration of the gums, while another was attacked with measles and purpura.

It was found that local applications, as borax and honey, muriatic acid, and sulphate of copper, had no appreciable influence on the disease; nor did the ordinary astringents, as chalk mixture, catechu, acetate of lead, or even opium itself, succeed in checking the diarrhœa. The best internal medicine was an acidulated decoction of bark, or an infusion of calumba with nitric acid. These medicines controlled in a remarkable degree the diarrhœa, which resisted other remedies. The most valuable remedial agent, however, was counter irritation produced on the abdominal surface. This was most easily effected by a mustard cataplasm kept on for an hour, by which time it produced free vesication, which was never followed by the unpleasant effects often observed after a cantharides blister. Wine was required in every case.

In another paper in the same journal by Dr F. Battersby "on some unusual complications and sequelæ of measles," the same diarrhœa and peculiar ulcerated state of the mouth were very commonly remarked. In many of his cases, however, this state was accompanied by asthenic inflammation of the pharynx, larynx, and bronchii, which often proved fatal after obstinately continuing for one or more months.

Dr Baly on the Influence of Confinement in producing Scrofula. (*Scrofula, its Nature, its Causes, its Prevalence, and the Principles of Treatment.* By Benjamin Phillips, F.R.S., &c.)

"In the statistical evidence which I shall adduce, I have been in many instances unable to distinguish between tubercular disease of the external glands, ordinarily denominated scrofula, and the tubercular disease of the lungs and other internal organs; but whenever it has been possible to show the prevalence of either form of the disease separately I have done so.

"The frequency of scrofula and internal tubercular disease amongst the prisoners in the Millbank Penitentiary, was one of the first and most important facts which offered themselves to my observation when I commenced my attendance at the institution in the spring of 1840. I found that the prevalence of the disease had already engaged the attention of the medical officers, and that a considerable number of prisoners who were most severely affected with it had been separated from the rest, and placed in a different ward, and that the system of discipline, as it regarded them, had by medical suggestion been much relaxed. The experience of the four subsequent years, and the examination of the medical records of the penitentiary, only strengthened the impression which I had at first received.

"The great amount of tubercular disease engendered by imprisonment in the penitentiary is proved by the following facts. During eighteen years, 205 deaths occurred amongst the prisoners in that establishment. Of this number, however, 31 deaths arose from the Asiatic cholera, which was epidemic during the years 1832–4, and only 174 from ordinary causes. Now of the 174 deaths, 75 were caused by consumption, and 8 by other forms of tubercular disease. Again, during the same period of eighteen years, 355 prisoners were pardoned on the ground of illness, and of these, 90 laboured under consumption, and 78 under external scrofula. So that very nearly half the deaths, and the same proportion of the pardons on medical grounds, were due to external or internal scrofula, or tubercular disease.

"The great mortality caused by this disease in the penitentiary is, however, best shown by comparing the number of deaths it has produced with the number of persons living, in the penitentiary and the whole metropolis respectively.

"Now the annual number of deaths from consumption during the period already mentioned, compared with the average number of prisoners, was in the proportion of 7.612 per thousand, and the annual number of deaths from other tubercular diseases in the proportion of .835 per thousand: while the pardons on account of consumption amounted to 9.386 per 1000 prisoners, and the pardons on account of scrofula to 8.135 per 1000. I believe we shall be very near the truth in admitting that three-fifths of the cases pardoned on account of consumption, and one-fourth of those pardoned on account of scrofula, would have proved fatal in the prison if no pardons had been granted; this at least is the estimate that I have formed after a very careful examination of the facts; and supposing it to be a correct one, we must add 5.361 to the proportional number of deaths from consumption, and 2.034 to the ratio of deaths from scrofula. We then find the total annual mortality from these diseases in the penitentiary to have been as fellows; from consumption, 13.244 per thousand prisoners, and from scrofula, 2.169 per 1000 prisoners. In the metropolis, during the year 1842, the mortality amongst persons of the ages of fifteen to seventy from consumption, was only 4.374 per 1000, and from other tubercular or scrofulous diseases, only .033 per 1000. The mortality from all tubercular diseases has therefore been nearly four times as great in the Millbank Penitentiary as in the metropolis. The excess of mortality from other diseases in the penitentiary has been comparatively slight.

"On examining the statistical reports of the penitentiaries of other countries, I have found that in them also the scrofulous or tubercular diseases have been the principal cause of death, and that the mortality from these diseases has been twice or three times as great among the prisoners in those situations, as amongst persons of the same period of life in the general population of the respective countries.

"That this great prevalence of tubercular disease amongst prisoners is the effect of imprisonment; that an extraordinary liability, of the persons forming the criminal class, to the disease, is not the chief cause, seems to be proved by the following facts.

"In the year 1840, 1052 prisoners were received into the Millbank Penitentiary, and of that number only 22, or not quite 21 per 1000 were affected with tubercular disease in one or other of its forms, namely, 14, or 13·3 per 1000, with external scrofula combined in 4 cases with consumption; and 8, or 7·6 per 1000 with the latter complaint without external scrofulous disease.

"Amongst 3249 male convicts who were received into Millbank prison during the year 1844, and whose state of health on reception is accurately recorded in the medical register, only 15, or 4·6 per 1000, were affected with consumption, and 44, or 13·5 per 1000, with scrofulous disease of the external glands. The proportion of consumptive persons amongst the convicts received in 1844 appears smaller than amongst those received in 1840, because the convicts in whom tubercular disease of the lungs existed only in a latent state at the time of their reception, are not included amongst the 15 recorded as consumptive in the register of 1844, while the 12 whom I have stated to have been affected with consumption at the time of their reception in 1840, comprehend not merely those in whom the disease was then in an active state, but also those in whom it first showed itself by marked symptoms within two months afterwards. The proportion of cases of external scrofula was very nearly the same in the two years.

"It must here be remarked, that all the prisoners sent to the penitentiary had been confined in the local gaols, for periods varying from a few days to several months, while many had been several times in prison; and that at least half of those affected with scrofula referred the commencement of the disease to some one of their previous imprisonments. The proportion of scrofulous and consumptive persons amongst criminals before their first imprisonment, must therefore be much less than would be inferred from the numbers above given. Still the proportion of cases of scrofula and consumption amongst the prisoners, even at the time of their reception into the penitentiary, appears small when we compare it with the number of cases developed within a short period after the commencement of their confinement there. 1030 was the number of those who were free from the disease when received in 1840; of this number, 510 were females under sentence of transportation, who remained on the average not more than three months in the penitentiary; and of these only two became scrofulous or consumptive during their sojourn there. The other 520 were penitentiary prisoners, who on the average were confined from two years to two and a-half years in the institution, and of these no less than 78 became the subjects of scrofula or consumption before the expiration of their terms of imprisonment. This fact appears to me conclusive evidence of the influence of imprisonment, or conditions attending it, in the production of scrofulous disease.

"The periods of their confinement at which prisoners labouring under scrofula and consumption have most frequently died or been pardoned, and the periods at which they have become affected with these diseases are also points worthy of attention.

"The proportion of deaths and pardons due to consumption and scrofula increase at first rapidly, and afterwards more slowly, as the periods of the convicts' imprisonment become more advanced. Thus, in the first year of their imprisonment, only 6.835 per 1000 of the number exposed to the chances of death or disease have died of these diseases, or have been pardoned on account of their labouring under them; in the second year the proportion has been 31.320; in the third year 49.848; in the fourth year, 52.373; and in the fifth year, 63.829 per 1000.

"Again, the number of new cases of consumption and scrofula compared with the number of prisoners amongst whom they occurred at different periods of imprisonment, has likewise increased as the periods were more advanced; the great increase, as we might expect, having taken place sooner here than in the case of the deaths and pardons. Amongst the prisoners, who when received into the prison in 1840, were free from scrofula and consumption, the new cases of these diseases which arose in the first six months of their confinement in the penitentiary, amounted to 10.701 per 1000 prisoners, but in the second six months to no less than 23.550 per 1000, and in the third six months to 43.859 per 1000. The number of new cases thus increased gradually during the first eighteen months; it then remained nearly stationary, or rather diminished. Now if imprisonment had produced no ill effects on the health, had exerted no influence on the development or production of scrofula or consumption, as many cases in proportion to the number of prisoners should have come under treatment in the first six months as in the second, third, and fourth periods of the like duration.

"It appears to me, therefore, that the predominance of consumption and external scrofula amongst the diseases of prisoners confined for long terms, must be regarded as the effect of this mode of punishment, or rather of the conditions which have hitherto generally attended it.

"The injurious influences which generally appear to be most active in producing scrofulous disease in prisons are, poorness and deficiency of food, defect of exercise, impurity of the air respired, want of external warmth, and depression of spirits.

"1. Poorness of diet cannot, it is true, be reckoned amongst the most active causes of scrofula in the Millbank Penitentiary; for the diet of this prison has since 1824 been very abundant, more so, at least, than the ordinary fare of the labouring classes, as is shown in the subjoined table.

"The chief respect in which the diet of the penitentiary has seemed calculated to favour the development of scrofula, is the being too little stimulating. At all events, a more generous diet, including a larger proportion of animal food, and beer, has in many instances had a beneficial influence in checking the progress of the disease in prisoners in the penitentiary.

"In many other British prisons, actual poorness of diet, especially its deficiency in animal food, has, I believe, been an active cause of scrofula. A marked difference in respect of their general health and the number of them affected with scrofulous disease, is presented by the convicts sent to the central prison at Millbank, preparatory to their transportation from different parts of Great Britain. By far the thinnest convicts, and those having the largest proportion of unhealthy and scrofulous individuals amongst their number, come from Scotch prisons, in which the diet consists of a sparing allowance of vegetables and farinaceous articles, and contains little or no animal food.

WEEKLY SUPPLY.

		Wheaten bread.	Meat, after cooking.	Potatoes.	Cheese.	Soup.	Gruel.	Milk and water, with flour.	Other allowances.
		oz.	oz.	lbs.	oz.	pints	pints	pints	
Dietary from Oct. 1824 to April 1830.	Men.	184	18	3	4	2	...	10½	1½ pint broth.
	Women.	138	15	3	4	2	...	10½	
Dietary from April 1830 to Jan. 1838.	Men.	176	24	4	2	2	...	10½	2 pints broth.
	Women.	132	20	4	2	2	...	10½	
Dietary from Jan. 1838 to July 1840.	Men.	184	20	5	4	1½	11	...	3 pints broth.
	Women.	138	16	5	4	1	7	5¼	ditto.
Dietary from July 1840 to July 1843.	Men.	176	25	4	2	1½	12	...	2 pts. broth, and 24 oz. boiled rice.
	Women.	138	16	3	4	1	7	7	ditto.

"2. All the best investigations of the causes of phthisis and scrofula hitherto instituted, have tended to show that defect of exercise or general muscular exertion, and the breathing of impure air are amongst the efficient causes of those diseases, and the extraordinary prevalence of scrofulous disease in prisons certainly is confirmatory of that result. For the majority of prisoners in most of the English gaols are kept without any occupation before trial, and after conviction are employed in work of a sedentary kind, such as oakum picking and tailoring. These employments, neither of which requires any strong muscular effort of the body, nor in fact any movement except of the arms, were the occupations of all the male prisoners in the Millbank Penitentiary, except of a few who were shoemakers, and of a still smaller number who were weavers. A certain number of them, it is true, were employed for a short time daily in turning the cranks of a machine for raising water, but this labour was only occasional, and the movements required by it were unvaried, slow, and spiritless. The exercise, too, which they were allowed to take for one and a-half, or two hours daily in the airing yard, was of the same character. They walked at regular distances from one another in a circle round each of the airing yards, generally at a slow pace, and with automaton-like movements.

"3. The breathing of impure air too, has I believe had a large share in the production of scrofulous disease amongst prisoners. The courts in which the prisoners in the penitentiary walk for exercise, are so inclosed by the prison buildings, that the air within them can with difficulty be changed. The air of these courts was formerly rendered impure by the foul air that escaped from the windows of the cells looking into them. The cells themselves, and the passages into which they open, were at that period very imperfectly ventilated; for the cells, though of good size, had no adequate provision for the renewal of the air within them. The occasional opening of the window or door allowed the air of the cell to be at times partially or entirely changed; but during many hours out of the twenty-four, and especially during the long night, the prisoners breathed air highly vitiated by their own breath. The smell, when the cells and passages were first opened in the morning, and at any time during the day when the doors had been shut for two or three hours, was exceedingly offensive. This state of things existed until 1841. In the course of that year a more perfect system of ventilation was partially introduced, and in 1842 was completed throughout the prison. By the new system of ventilation, a constant renewal of the air of the cells and passages is effected, quite independently of the caprice of the prisoners, the direction of the wind, or other uncertain influence. An improvement in the health of the prisoners followed this change, and a smaller proportional number became scrofulous. But this improvement cannot be wholly ascribed to the improved ventilation; for other important changes in the condition of the prisoners were also made in the years 1841 and 1842. The benefit derived from breathing a purer atmosphere was, however, frequently observed in the penitentiary in the case of those prisoners who were already affected with scrofula. They were allowed to spend three or four hours daily in the garden on the outside of the prison buildings, and in almost every case their health was improved, and their disease temporarily, if not permanently checked. Here also, however, it is not possible to say how much of the benefit derived was due to the better air. For these prisoners while in the garden were employed in active labour; they also were allowed half a pint or one pint of beer, two pints of milk, and half a pound of mutton daily, and wore under garments of flannel. These were undoubtedly causes contributing to the improvement of their health. In most prisons the ventilation of the cells occupied by prisoners has until very recently been exceedingly defective.

"4. The next of the causes to which the prevalence of scrofula in prisons seems to be due is external cold. It will not I think be difficult to demonstrate the powerful influence of this cause in the production of scrofulous disease.

"In the Millbank Penitentiary the sufferings of the prisoners from cold during the year were extreme. Ample means of warmth were provided in the building; so much hot air, indeed, was thrown into the passages as to render them frequently warmer than was required for the sake of health; but this hot air had no access to the cells, except through the crevices round the doors, the small opening provided for the purpose of inspection, and in some instances another small opening near the ceiling. Very little warmth could enter by these ways, so that the air of the cells, when not occupied by prisoners, was but little warmer than the external atmosphere. It is not surprising, then, that persons for the most part not of robust constitution, cut off from stimulating food and all spirituous drinks, having no extra clothing, breathing an impure air, often depressed in spirits, and above all, engaged in sedentary occupations, which required no muscular exertion,—in a word, subjected to almost every influence which has a tendency to render the circulation languid, and the oxygenation of the blood imperfect—it is not surprising, I say, that such persons should, in consequence of confinement during the winter in these cells, have presented in an extreme degree the injurious effects of cold. These effects were not such as result from occasional sudden exposure to the influence of cold; not acute inflammation of internal organs, but such morbid conditions as we might expect to be produced by the long continued action of a low temperature. The prisoners, with scarcely any exception, lost flesh during the winter; and a very large proportion of them suffered severely from rheumatic pains, and still more from chilblains, and their whole appearance betokened the languid state of their circulation. Such a state of the system was likely to favour the deposition of scrofulous matter in persons predisposed to scrofulous disease; and it is certain that the first symptoms of scrofulous disease, in by far the larger number of instances, appeared during the cold season of the year, and that the disease, if previously existing, always became aggravated at that season.

"My own observation of the health of the prisoners in the penitentiary during several years has convinced me of this fact; and the register of deaths and pardons on medical grounds, which has been kept in the institution during a long series of years, demonstrate it in a striking manner. In every case of death, or pardon on account of impaired health, the date of death, or of the reception of the pardon, as well as the cause of death, or ground of the medical recommendation for pardon, has been accurately recorded. By the analysis of this register, therefore, I have been able to ascertain the numbers of the deaths and pardons due to scrofulous disease which have occurred in the different months of the year respectively. Now scrofula is a disease, for the most part, of slow growth and progress; consequently if it were produced, or greatly aggravated by the cold in the winter, it would in most cases not reach a fatal termination, or a stage so advanced as to threaten life until the spring, or the early part of the summer. This is what has actually been the case. The greatest number of deaths and pardons due to scrofulous disease occurred in the penitentiary during the spring and summer months, the smallest number during the autumn and winter. Thus while the number of deaths and pardons during eighteen springs and summers was 175, the number during eighteen autumns and winters amounted to only 74. During the Mays and Junes of eighteen years, there were 79 deaths and pardons due to scrofula and consumptive disease; during the Novembers and Decembers of the same eighteen years only 20.

"This great difference between the different seasons as regards the number of deaths and pardons due to consumption and scrofula, which is observed also under other circumstances, but is nowhere, I believe, so marked as in prisons, appears to me to be explicable in no other way than by regarding it as the effect chiefly of difference of temperature. The greater foulness of the air breathed by the prisoners during the winter, when they, in the endeavour to exclude the cold air from their cells, kept their windows continually closed, most probably contributed to the result, but did not in my opinion produce nearly so much of the difference in the mortality of the different seasons, as did the direct influence of cold.

"5. The last cause which I have mentioned as giving rise to the great prevalence of consumption and scrofula in prisons, is a depressed or listless state of mind. It will, I think, be readily admitted that mental depression, aided by other circumstances, is capable of producing these diseases, or at least of aggravating them when they exist. And although there may not have been much of absolute despondency or remorse amongst the prisoners in the penitentiary, yet there was a state of mind not less injurious; I mean a listless and torpid condition, an absence of all cheerful and varied thought, attended, in most cases, by an uneasy and anxious sense of restraint, and desire of liberty. The influence which this state of mind had exerted, became most apparent when it was suddenly removed. Prisoners who were even in an advanced stage of consumptive disease, and who in the infirmary had been gradually and rapidly getting worse, immediately improved on being released from confinement; and in many instances, I have observed this improvement in their symptoms to commence as soon as the fact of their being recommended for pardon was communicated to them, which happened sometimes two or three weeks before their discharge.

The amelioration which began under these circumstances, was often not of a transitory nature; but the disease which a short time previously seemed to have reduced the patients to a hopeless condition, was permanently checked. I have had several opportunities of satisfying myself of this fact, and could instance five cases in which prisoners who had been discharged from the penitentiary while labouring under fully developed pulmonary phthisis, with indubitable signs of softened tubercles, and even tubercular excavations, have, in consequence of fresh crimes, been sent again to that prison, and have then not merely appeared, and felt themselves to be, in perfectly restored general health, but have offered only the most obscure, if any, physical signs of structural change existing in their lungs. In all these cases, however, the renewed exposure to the injurious conditions attending imprisonment, called their diseases again into activity. With external scrofula, this has been even more frequently the case. Criminals who have been several times in prison, have often stated that they became affected with scrofula during their first imprisonment, that they got entirely rid of their disease when they obtained their liberty, but that it returned soon after they were a second time committed to prison; that the same mitigation of the disease attended each successive discharge from confinement, the same renewal or aggravation of it, each fresh imprisonment.

" Many of the facts which I have stated in general terms in this letter are given in a more detailed and statistical form in the accompanying tables. The 12th table, which illustrates the influence of age on the frequency of tubercular disease amongst criminal prisoners, will probably be of some interest to you, and requires no explanatory remarks to render it intelligible."

TABLE I.

Showing the numbers of pardons granted to prisoners in the Millbank Penitentiary on account of consumption, and hæmoptysis, scrofula, and all kinds of diseases during the eighteen years, 1825 to 1842 inclusive; also the ratio per 1000 of these pardons to the average number of prisoners.

	Cases of pardoning on account of consumption and hæmoptysis.		Cases of pardoning on account of tubercular diseases*		Cases of pardoning on account of all kinds of sickness.	
	Number of pardons in 18 years.	Annual ratio of pardons per 1000 prisoners.	Number of pardons in 18 years.	Annual ratio of pardons per 1000 prisoners.	Total number of pardons in 18 years.	Annual ratio of pardons per 1000 prisoners.
Male prisoners.	68	9.362	51	7.022	213	29.326
Female prisoners.	22	9.462	27	11.613	142	61.075
Both sexes.	90	9.386	78	8.135	355	37.025

* All were cases of scrofula, combined in many cases with internal disease.

TABLE II.

Showing the number of prisoners, of different classes, amongst those received into the Millbank Penitentiary in the course of the year 1840, who were affected with scrofula and consumption at the time of their reception; also the number of prisoners amongst those who were received in 1840, who became affected with these diseases before the end of 1843.

	Number of prisoners received in 1840.	Number of prisoners, who when received in 1840, were affected with				Number of prisoners apparently free from these diseases when received in 1840.	Number of prisoners (out of those apparently free from these diseases when received in 1840) who became affected with them before the end of 1843.			
		Scrofulous disease of the external glands.*	Pulmonary consumption.†	Both pulmonary consumption and external scrofula.	Total.		Scrofulous disease of the external glands.	Pulmonary consumption.	Both pulmonary consumption and external scrofula.	Total.
Male penitentiary prisoners.	432	3	6	2	11	421	15	38	9	62
Female ditto ditto	100	—	1	—	1	99	8	3	5	16
Females under sentence of transportation	520	7	1	2	10	510	2	—	—	2
All classes	1052	10	8	4	22	1030	25	41	14	80

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* Observed at the time of the prisoners' reception.

† Detected when the prisoners were received, or found soon afterwards to be in so advanced a stage, as to leave no doubt as to the disease having existed at the time of their reception.

TABLE III.

Showing the number of male convicts who were received at the Millbank Prison during the year 1843, and whose state of health is recorded in the Medical Register; and the number amongst those who were at the time obviously affected with scrofula or consumption, or had scrofulous cicatrices on their necks.

Total number received.	Number who had scrofulous disease of the external glands.	Number who had existing symptoms of pulmonary consumption.	Number who had cicatrices of scrofulous sores on the neck.
3249	44†	11*	27

TABLE IV.

Number of deaths and of pardons, on account of consumption and scrofula during the 17 years, 1825–41.

Periods of imprisonment.	1st 3 months.	2d 3 months.	2d 6 months.	3d 6 months.	4th 6 months.	3d year.	4th year.	5th year.
Number exposed to the chances of disease or death in each period of imprisonment.	3571	3470	3264	2905	2472	1645	611	94
	3365			2682				
Number of deaths and pardons due to consumption and other tubercular diseases in each period.	...	2	21	43	41	82	32	6
Annual ratio of deaths and pardons per 1000 prisoners at each period of imprisonment	...	2.305	12.867	29.604	33.172	49.848	52.373	63.829
	6.835			31.320				

TABLE V.

Proportion which the cases of consumption and scrofula bore to the number of prisoners in each period of imprisonment.

Periods of imprisonment.	1st 6 months.	2d 6 months.	3d 6 months.	4th 6 months.	5th 6 months.	6th 6 months.
Mean number exposed to the chances of becoming diseased in each period . .	841	552	456	418	359	255
Cases of consumption and scrofula which occurred in each period . . .	9	13	20	14	15	9
Ratio per 1000 prisoners of the cases of consumption and scrofula in each period .	10.701	23.550	43.859	33.492	41.782	35.293
Annual ratio per 1000 prisoners	21.402	47.101	87.719	66.985	82.565	70.587

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* In four out of this number, signs of tubercular disease in the lungs were detected, and these four had also had hæmoptysis.

† Two of these had cicatrices of scrofulous sores on the neck.

TABLE VI.

Showing the number of deaths and pardons due to consumption and scrofula which occurred amongst the prisoners in the Millbank Penitentiary in the different seasons of the 18 years, 1825 to 1842 inclusive.

	18 Springs. March, April, and May.	18 Summers. June July and August.	18 Autumns. Sept. Oct. and Nov.	18 Winters. Dec. Jan. and Feb.
Number of deaths from consumption and scrofulous disease . .	31	28	12	10
Number of pardons on account of consumption and scrofulous disease	58	58	17	35
Number of deaths and pardons together . .	89	86	29	45

TABLE VII.

Showing the ages of the 3777 penitentiary prisoners received into the Millbank Penitentiary, from October 1824, to the end of 1842, and the number of deaths and pardons due to consumption and scrofula which occurred amongst the prisoners of the different ages.

AGES.	Under 15 years.	15 and under 20 years.	20 and under 25 years.	25 and under 30 years.	30 and under 40 years.	40 and under 50 years.	50 years and upwards.	Total.
No. of prisoners of each age when received . .	296	1196	901	535	539	198	112	3777
No. who died of consumption or scrofula, of those received at the respective ages	5	30	20	10	8	6	2	81
Ratio of deaths per 1000 prisoners received at the respective ages .	16.89	25.08	22.19	18.69	14.84	30.30	17.85	21.44
No. who were pardoned on account of consumption or scrofula of those received at the respective ages.	14	61	46	23	17	6	1	168
Ratio of pardons per 1000 pris. at the respective ages	47.29	51.00	51.05	42.99	31.54	30.30	8.92	44.48
Deaths and pardons due to consumption and scrofula amongst the prisoners received at the respective ages	19	91	66	33	25	12	3	249
Ratio of deaths & pardons per 1000 prisoners of the respective ages when received . . .	64.18	76.08	73.24	61.68	46.38	60.60	26.77	65.92

Can Syphilis be communicated to the Lower Animals? By M. CULLERIER. (*Archives Generales de Medecine*, May 1845.)—M. Cullerier having great doubts as to whether the ulcers on the face of the ape, which M. Anzias exhibited to the Chirurgical Society of Paris, really resulted from syphilitic inoculation, he made a great number of experiments to ascertain whether human syphilis can be communicated to the lower animals. In his paper on the subject, M. Cullerier relates the history of eight inoculations of guinea pigs with syphilitic virus, both by pricking and by incision, but all without communicating the disease. Syphilitic virus was in various ways introduced into wounds on the bodies of rabbits, dogs, the cat, and ape, amounting in all to 25 inoculations, but without in any instance communicating the disease. The wounds healed as quickly as if they had been simple incised wounds. The virulence of the matter used was in many cases tested by inoculating with it parts of the body of the persons from which it was taken, when it produced the characteristic sore. The want of result in all these cases led him to invite M. Anzias to endeavour to repeat his experiments on the animals which were the subject of the above attempts. He accordingly repeated them in the presence of M. Cullerier on the dog and ape, and though every precaution was used, and the wounds were irritated and re-opened and fresh matter applied, no syphilitic sore was produced. M. Cullerier, however, found that ulcers precisely similar to those which M. Anzias had exhibited on the face of the ape, and termed syphilitic, could be produced by repeated applications of nitrate of silver or the actual cautery to the skin of any of the lower animals. The base of these ulcers was indurated, and the surface presented the peculiar aspect of syphilitic sores. Besides, the ape which was the subject of M. Anzias experiment fell a victim to phthisis, and not to syphilis, as the minute examination of its body showed, for tubercles were detected in all their varied stages in the lungs, liver, spleen, mesentery, and intestines. From all this M. Cullerier concludes that "syphilis in all its forms is still the exclusive sad portion of the human race."

MATERIA MEDICA AND THERAPEUTICS.

Instructions for making Unfermented Bread, with Observations, by a Physician. London, 1846, 8vo, pp. 15.—In the year 1816, Dr Thomas Thomson published, in an article on Baking in the Supplement to the Encyclopædia Britannica, the following directions for making bread without yeast, yet light and spongy. Supposing a quartern loaf to contain one ounce of salt, or 437 grains of muriate of soda, then a method of substituting for the salt the chemical elements of that substance is the following: Dissolve in water two ounces five drachms and forty-five grains of crystallised carbonate of soda, and mix the solution well with dough; add then seven ounces two drachms and twenty-two grains of muriatic (hydrochloric) acid, of sp. gravity, 1.121, and knead rapidly. It will rise immediately, fully as much, if not more, than dough mixed with yeast.

To this prescription for making bread without yeast not much attention was given for many years, although it is unquestionably elegant, simple, and, within certain limits, effectual. Within the last two or three years, however, several bakers have announced to the public that they prepared a species of unfermented, or, strictly speaking, unyeasted bread, which they represented to be light, very digestible, and free from the acescent properties and effects which have been generally ascribed to bread fermented by yeast, and leavened. This bread, also, they have distinguished by the name of Patent Bread.

The author of the present tract states that the method was at the time tested by several other persons, and found to answer perfectly, but that it was regarded in the light of a philosophical curiosity.

It always appeared to us that the proportion of hydrochloric acid recommended by Dr Thomson was too great, and, from recent trials, we are convinced that this is the case. If we calculate, in any of the ordinary systems of chemistry, the proportions in which hydrochloric acid should be united to soda in order to form salt, it will be found that seven ounces of hydrochloric acid is too large a quantity to be united with two ounces and a half of carbonate of soda.

Without adverting to this fact, the present author publishes a formula, which he has found by experience to answer best, and in which the proportion of hydrochloric acid is reduced to about one-third of that which it is in the formula of Dr Thomson. The new formula is as follows, for white bread and brown bread:—

White bread.

Take of flour, dressed or household, 3 lbs. avoirdupois; bicarbonate of soda, in powder, half-an-ounce Troy, or 4 drachms; hydrochloric acid (sp. gr. 1·17) 5 fluid drachms; water, about 26 fluid ounces; salt, two-thirds of an ounce Troy, or 4 drachms.

Brown bread.

Take of wheat meal 3 lbs. avoirdupois; bicarbonate of soda, in powder, 4½ drachms; hydrochloric acid (sp. gr. 1·17) 5 fluid drachms and 25 minims; water, about 30 fluid ounces; salt, two-thirds of an ounce Troy.

First, mix the soda and flour as thoroughly as possible, which is best done by shaking the soda from a small sieve over the flour with one hand, and stirring the flour all the time with the other. This answers, in general, well; but the equable distribution of the soda through the flour, which is important, is most certainly attained by passing the mixture once or twice afterwards through a sieve. Next, dissolve the salt in the water, and add to it the hydrochloric acid, stirring well with a wooden or glass rod. Then mix the whole intimately and perfectly as speedily as possible, stirring well by a wooden spoon or spatula. The dough thus formed makes two loaves somewhat larger than half-quarterns. They should be placed in the oven without loss of time. The author recommends them to be baked in long tins, and the oven to be much hotter than for common bread. The tins are not absolutely necessary. The loaves may be baked in ordinary fire-proof earthen-ware dishes. About one hour and a half is required for the baking.

The author states that the proportions of soda and acid are those which, when united chemically, make common salt. To this they come nearer than the proportions given in the formula by Dr Thomson. But we have found by trial, that even a little more soda may be used to saturate the acid thoroughly, and if not used the bread is rather acid. The author himself admits that an excess of soda does much less harm than an excess of acid. The union takes place immediately, and, at the same time, carbonic acid is extricated from the carbonate of soda. But this extrication does not take place so as to cause the dough to rise immediately, as Dr Thomson represents, but chiefly after and during the application of heat. The dough then rises; and as it is expanded by the carbonic acid extricated, it is converted into spongy, porous, and comparatively light bread.

The water should be used cold, and care should be taken not to use too much water.

Such is the process used for making patent bread, or bread porous, yet without being leavened or fermented. The bread thus baked is well tasted, and it is said to be lighter and more digestible than bread baked with yeast. It is certainly more dense, not quite so porous, and we think, from observing its formation several times, that it is not so perfectly and thoroughly raised or inflated as bread baked with good yeast.

Bread baked in the manner now described contains nothing but flour, salt, and water; and it is thought by its patrons that, being made without yeast, it is less acescent. This is not universally true. Bread baked with yeast of inferior quality is certainly rendered more or less sour, or is liable to become sour; but this seems to be the fault of the process or mode of making the yeast, and not that of the baking. Many of the complaints brought against fermented bread are manifestly applicable to the yeast with which it is fermented, or the mode in which the fermentation is carried on.

It is said that the Edinburgh bread is much better baked than the London bread; and the French bread is said to be better than either. But there is no bread equal to the Belgian; and, supposing the flour to be nearly the same, this superiority is to be ascribed chiefly to the superiority of the Belgian and Dutch yeast.

The author of the present tract is of opinion that the unyeasted bread is much more salubrious and more safe for the dyspeptic than the fermented bread of the baker. And he farther thinks, that bread baked from wheat meal, or unboulted flour of wheat, is much more nutritious and salubrious than bread baked from fine white flour. It is impossible to deny that the different processes to which the ground powder of wheat is subjected, are not only unnatural, but in many respects uneconomical. By screening, sifting, and boulting, not only is the bran removed, but a good deal of what might be used as wholesome food is lost. By the practice of separating the fine flour, and baking only that, great waste is caused, and it is doubtful if the practice be not hurtful to digestion. On the other hand, it must not be denied that it is impossible, either by yeast or by carbonate of soda and hydrochloric acid, to raise bread from brown or wheat meal, so completely as that from flour which has been screened, sifted, and boulted.

In the northern counties of England, as Yorkshire and Northumberland, it is the practice to employ, in the manufacture of bread, the mere ground wheat—that is, wheat ground and the coarsest part only separated,—and the bread so baked is palatable, light, and nutritious.

The great purpose of the present author in recommending the use of brown unfermented bread, is to obviate constipation and to diminish the violence of dyspeptic symptoms, or entirely remove them. He has been himself an invalid, and speaks with the weight which experience gives.

Another argument which he employs in its favour is, that, by the process of separating, screening, and refining, there is incurred a loss of 25 per cent. We have no doubt that great loss is incurred; and surely, in an article so necessary, it would be most prudent to employ the flour as formed from wheat in the most simple, natural, and economical mode.

On the Treatment of Seminal losses by Compression. By M. BRACHET. (*Bulletin de l'Acadamie Royale de Medecine,* May 1845.)—M. Brachet related four cases of spermatorrhœa depending on different causes which were cured by means of compression applied to the prostate gland behind the perineum. He attributed the success of the treatment to two causes: *First,* the keeping the semen in its reservoirs and accustoming them to bear its presence; and, *second,* to the compression modifying the physiological condition of the urethra, prostate gland, and secreting organs. The apparatus he employed was very simple. A belt of leather was passed round the waist, from the back of which a strap descended to pass over the perineum between the thighs. This strap was in one piece behind, but was split into two heads in front, so as to allow room for the penis, &c. A moveable compress was attached to that portion of the strap which was over the spot on which it was desirable compression should be exercised, and the whole was tightened by means of the two heads of the strap being brought up before and fixed to the belt in front. He condemns the practice of applying compresses or rings to the penis for the prevention of involuntary seminal ejaculations, as they do not cure the disease nor prevent the semen being thrown into the bladder.

Influence of the Quality of the Milk of the Nurse on the Health of the Child. By M. Girard. (*Journal de Pharmacie et de Chemie*, October 1845.)—A young lady, fifteen days after her delivery, found it necessary to procure a nurse for her child, and she succeeded in getting one whose milk was only fifteen days older than her own, and which presented all the physical characters of good milk. The nurse was besides young and in apparently good health. After a few days the child began to sleep ill; it was attacked with a troublesome diarrhœa, and sensibly fell off; shortly after this it rejected all its food, and the mouth got foul with aphthæ. As the ordinary medical treatment failed to relieve the child, the nurse's milk was examined, and was found to present the following characters:—The milk was neuter, of an ordinary consistence, and became slightly viscous by ammonia. The globules were very voluminous, and instead of appearing under the microscope of a brilliant pearly lustre, were of a dull white opal colour. The whole field of the microscope was scattered with rounded particles, of which the form was comparable to that of a flattened raspberry, and presented the characters usually assigned to the corpuscles of colostrum. A new nurse was, therefore, instantly procured, whose milk presented the microscopic properties of healthy milk. Within forty hours after the child had sucked this nurse the vomiting and diarrhœa had ceased, the aphthæ were greatly diminished, and a few days sufficed to restore the child to strength.

In another case where the mother was the nurse, the child, two months old, suffered from constant vomiting, was falling off, and had occasional comatose attacks. The milk of the mother, when examined by the microscope, was found to contain a considerable quantity of mucus. After a while the child vomited less, and the milk being anew examined was found to contain fewer mucous globules. Soon, however, the vomitings and other disagreeable symptoms became worse, and to save the child it was absolutely necessary to change the nurse. Eight nurses were examined, but the milk of seven was rejected on account of containing globules of mucus, or granular bodies, or from having undergone some other change. The eighth nurse having apparently healthy milk, was chosen, and within two days the vomiting and other symptoms had disappeared, and did not again return.

SURGICAL PATHOLOGY AND THERAPEUTICS.

Successful case of the employment of Iodine in removing sparks of Iron from the Cornea. By M. Reiniger. (*Journal de Pharmacie et de Chimie*, June 1845.)—A small particle of steel struck the eye of a cutler while at work, and fixed itself in the thickness of the cornea, from which it was found impossible to remove it either by the forceps or needle. It soon excited inflammation of the cornea, and after eight days the eye was still red, painful, hot, and the patient complained of a pricking sensation on its being moved. The fragment of steel could be distinctly seen, still retaining its polish. As a strong magnet also failed to remove it, it was resolved to have recourse to a chemical solvent, but one which would not act deleteriously on the eye. A weak solution of iodine and iodide of potassium was therefore used as a collyrium, and its very first application sensibly deadened the lustre of the spark of steel. By its continued use the steel was rendered soluble, and was gradually removed when the sight was restored. The treatment was therefore most successful, and seemed to be preferable to that formerly recommended, viz. the employment of a dilute acetic acid, which is apt to impair the vision.

Means of avoiding the great mortality following the great operations of Surgery. By M. Ballard. (*Journal de Pharmacie*, July 1845.) M. Ballard, surgeon to the Military Hospital at Besancon, read a paper on this subject to the Academy of Sciences. He observed that the mortality among patients who, previous to submitting to operation, were kept on a generous diet, was very nearly the same as in those who had previously been reduced by low diet and regimen; the only difference being that the first died from the fifth to the tenth day after the operation, while the latter died from the thirtieth to the fortieth day thereafter. By trying to steer a middle course between these two extremes, he found that the mortality was increased. He then set himself to study the causes of the mortality, and traced it to five causes. 1. The fear of the operation, and the anxiety which precedes its performance. 2. The pain; it is to the shock given to the nervous system that many patients die either during the operation or shortly after. 3. Traumatic fever or suppuration. 4. Purulent infiltration of the tissues which have been inflamed; and lastly, Putrid and unhealthy exhalations which arise from particular localities. To neutralise these so far as possible, he employed the following means. 1. He kept the patient in perfect ignorance of even the necessity for operating till the moment of operation. 2. He deadened the pain either by means of a ligature pretty tightly applied, or diminished the sensibility by means of large narcotics given two or three successive days before the operation. 3. He prevented fever or inflammation of the part by the local application of cold, which had also the effect of preventing purulent infiltration; and lastly, he used the ordinary precautions to prevent exposure to unhealthy exhalations. By acting on these principles, he has had an amount of success quite astonishing. Of 28 amputations, 11 of which were of the thigh, he only lost a single patient.

On Calculi of the Nasal Fossæ. By M. Demarquay. (*Archives Generales de Medecine,* June 1845.)—The occurrence of a case of calculus of the nasal fossa in a patient admitted into the Hotel Dieu, under the care of M. Blandin, attracted M. Demarquay's attention to the subject. The subject of this case was a woman, 35 years of age, who, for a couple of months, had been troubled with considerable impediment to breathing through the left nostril. For some time past fetid purulent matter had been discharged from that nostril. She brought with her a calculus about the size of a pea, which M. Barth had extracted a few days before. She remained four days in the hospital, during the first three of which a few very small calculi, varying in size from a pin's head to that of a pea, were abstracted. On the third day one of the size of a bean was removed. Its surface was rough, and on being sawed through its nucleus was found to consist of a cherry stone. On analysis these calculi were found to consist of phosphate of lime and magnesia, carbonate of lime and of magnesia, chloride of sodium, with traces of the carbonate of soda.

On examining the literature of this very rare disease, M. Demarquay found that fourteen cases had been recorded in which calculi of various sizes escaped from, or were removed, from the nasal cavities. The following is a short abstract of these cases.

1. Bartholin in 1654 stated that a young girl of distinction, when blowing her nose, expelled several calculi, among which was one of the size of a date stone.

2. The same author relates, that a woman of Helsimbourg by mistake swallowed a cherry stone, which remained several weeks in the fauces. It occasioned acute pain, and the formation of a tumour, which, however, did not suppurate; but one night, in a fit of coughing, the cherry stone was ejected, and was found covered with a pretty thick coating of a calcareous looking substance. The tumour after this rapidly disappeared.

3. Clauder in 1685 related the following case. A woman, 60 years of age, subject to colds, began to be annoyed with an abundant discharge of thin mucus from the right nostril, with difficulty of breathing through it. A resisting object was felt on probing the nostril. During a fit of sneez-

ing this body got dislodged, and was extracted. It was the size of a walnut, and was so hard that it could with difficulty be broken with the hammer.

4. Kerne in 1700 published a case in which acute headachs were caused by nasal calculi. A young girl for about a year and a half had been subject to obstinate frontal headach, for which various remedies were used without success. Kerne ordered a sternutatory, which had scarcely began to act before he was called in haste to see her as she was threatened with suffocation from some body obstructing the nostrils, in which also violent pain was felt. More of the sternutatory was given, when a calculus of the size of a pea was expelled, followed shortly by several others. After the expulsion of the calculi the headachs disappeared.

5. Vitus Riedlinus in 1706 stated that a young married woman suffered from obstruction of one of the nostrils, which was swollen and emitted a most offensive odour. On probing the nostril, a hard substance was felt, which was seized with the polypus forceps and removed. Several calculi were thus removed, the largest the size of a bean. From this moment the patient recovered.

6. Wepfer in 1727 published the history of a calculus which he removed from the right nostril of an old lady in 1680. It obstructed the nostril and caused a considerable mucous flow from it. It was situated in a large ulcerated hollow tumour, resembling a polypus, which filled the nostril. The tumour appeared to be connected with the roots of a carious front tooth, as pulling on it caused the movement of the tumour, and the calculus resembled the incrustation which forms on the tooth, but was harder.

7. Ruysch in 1733 related that a young girl of five introduced into the nostril a piece of amber. It gave considerable annoyance till she was fourteen years of age, when, in a fit of sneezing, it was expelled, surrounded with a thick coating of cretaceous matter.

8. Horn in 1788 published a case in which a cherry-stone, which escaped into the nostrils, gave rise to symptoms of polypus, in consequence of becoming encrusted there with a voluminous coating of cretaceous-looking substance.

9. Saviales in 1814 published the history of a case in which a man, 42 years of age, suffered from continued headachs and pain in the nostril. After an attack of erysipelatous inflammation, an abundant purulent secretion was discharged from the nostril, which seemed to be filled with a polypus. On seizing it, it turned out to be an earthy concretion an inch and a-half long, which filled the nostril, and on being broken was found to contain as a nucleus a cherry-stone.

10. Graeffe in 1828 related a case in which an earthy concretion formed in one of the nostrils, not only giving rise to frontal pain, but also serious affection of the corresponding eye. Before the concretion was discovered and removed, the tears appeared to possess a caustic acid nature; and M. Graeffe thought that the calculus was produced from matter deposited by them, modified as they probably were by the person possessing an arthritic constitution.

11. In another case narrated by the same author, in which tolerably similar symptoms were present, the calculus, which was of an oval form and about an inch long by half-an-inch broad, contained a cherry-stone as a nucleus, around which the calcareous matter was deposited.

12. M. Flouret in 1829, in dissecting a subject who died at the Hospital of la Charite, found a nasal calculus of an inch and a-half in diameter occupying the right nostril.

13. In 1829 Dr Axman met with a curious case, in which severe attacks of hemicrania occurred periodically for many years, but were not amenable to treatment till after taking a pinch of snuff, when a calculus of the size of a bean was discharged. The hemicrania disappeared, but after a time

returned, when severe fits of sneezing artificially produced, caused the evacuation of a large number of calculi with much fetid purulent matter. From this period the patient completely recovered. These calculi on analysis were found to consist of animal matter, 3.5; phosphate of lime, 8.0; carbonate of lime, 32.5; carbonate of magnesia, 12.5; with a trace of muriate of soda and oxide of iron.

14. Brodie published in 1844 the history of a case in which from infancy there had been an offensive discharge of matter from the nostril, with the usual symptoms of diseased bone; and the case was accordingly treated as such. One day, when about twelve years old, in blowing her nose, a calculus escaped, which on being examined by Dr Prout, was found to be composed of inspissated mucus penetrated with phosphate of lime.

Necrosis of the Maxillary Bones produced by the vapours of Phosphorus. By Professor HEYFELDER of Erlangen in Bavaria. (*Archives Generales de Medecine*, October 1845.)—Some time ago M. Lorinser, head surgeon of one of the civil hospitals in Vienna, described a peculiar necrosis of the maxillary bones. The persons affected were women varying in age from 19 to 40 years, who had been employed from four to nine years in lucifer match manufactories, to dry the matches after they had been dipped in the phosphoric compound. This process was conducted in large chambers usually filled with the vapours of phosphorus. The disease commenced by toothach more or less severe, at first confined to one tooth, but afterwards extending over the whole jaw, which swelled and became painful to the touch. After a while the cheeks, the gums, and the neck participated in this painful swelling. At the same time acute fever came on, accompanied by ardent thirst and copious salivation, while the skin assumed a straw-yellow hue. The affected teeth loosened, fetid purulent matter exuded from the sockets and gums; after this the teeth fell out, the soft parts parted from the maxillary bone, and left it exposed. Young and strong women in whom this disease had only gone on a little way recovered after exfoliation of the maxillary bone; but all of a weak or scrofulous constitution died by *phthisis pulmonalis*. Nine cases in all were observed at Vienna furnished by different manufactories.

In five of these the disease affected the upper jaw; in the remaining four the lower jaw was the part seized. Of the nine cases five died, one was cured, and three were in a convalescent state.

This singular affection of the maxillary bones has also been observed at Nuremberg among the workers in the lucifer match manufactories. Nine cases were furnished by one manufactory alone. The patients were women from 18 to 27 years of age, some married, others unmarried. They had all previously enjoyed good health, and had spent about fifteen hours daily in the manufactory of the phosphoric matches. In four the disease attacked the upper jaw; in the remaining five the lower jaw was affected. Only two recovered; one after the spontaneous separation of the alveolar portion of the upper jaw, the other by having the lower jaw removed by a surgical operation. Six died, two of whom had the lower jaw removed by operation. In one case which was examined the coronoid and condyloid processes were healthy; but in another these parts were covered with a thick greyish-coloured osseous layer of unhealthy aspect.

M. de Bibora analysed the new layer of bone thrown out in the above case, and found that the organic and fatty matters existed in considerably larger proportion than in sound bone. No free phosphorus, however, was noticed in this bone.

Case of Vagino-labial Hernia. By Professor STOLTZ. (*Gazette Medicale de Strasbourg*, January 1845.)—During the month of January 1844 M. Stoltz had occasion to examine a pregnant woman, who, whilst coughing at stool, suddenly felt a large swelling make its appearance in the right labium. The tumour diminished when the patient lay down, but became painful and very large when she stood, or was engaged in fatiguing occupations. The tumour disappeared completely under compression, but reappeared on coughing. These characters enabled it to be recognised as a hernia, but M. Stoltz ascertained it was not an inguinal one, for the hand pressed over the inguinal canal and aperture did not prevent the hernia from descending into the labium. It was remarkable that in this case the hernia did not return after delivery.

In commenting on this case, M. Stoltz confirms the propriety of Sir A. Cooper's naming this form of hernia pudendal hernia; and he considers it to be quite distinct from perineal hernia, with which it might be confounded. The much greater breadth of the perinæum in women favours the occurrence of hernia in them. But the broad ligament of the uterus divides the pelvis into two portions, a posterior and an anterior. If a loop of intestine descends in front of this broad ligament along the side of the vagina, and if it pushes before it or separates the fibres of the *levator ani*, the hernia will occupy the labium and nowhere else. But if, on the other hand, the loop of intestine descends behind the broad ligament, it descends between the vagina and rectum, and shows itself in the perinæal region near the margin of the anus. The hernia is then a perinæal one.

Two cases of Prolapsus of the Uterus cured by excision of a portion of the vagina. By M. Leloutre. (*Archives Generales de Medecine*, October 1845.) A married woman, 45 years of age, the mother of five children, suffered from *prolapsus uteri* brought on by a fall on the fundament. Pessaries gave intolerable pain, and for several months the woman was in a deplorable state. Between the thighs there was a tumour of a pyriform shape, 11 inches 9 lines in circumference, and 5 inches in height, composed of the uterus with a portion of the vagina. Three longitudinal portions of the mucous surface of the vagina, extending from the *os uteri* to the vulva, were cut out, one on each side and one behind. The edges of the wounds were then cauterized and the uterus replaced in the vagina. No unfavourable symptoms followed. A month after cicatrisation was complete, the vagina was considerably narrowed, and the uterus remained *in situ*. Forty-four days after the operation the patient got out of bed, and at the end of eighteen months the prolapsus had not returned.

The second case was a woman 50 years of age who had led an irregular life, and who had a complete prolapsus of the uterus with inflammation. After the inflammatory symptoms were subdued four longitudinal portions of the mucous surface of the vagina were removed, and the wound treated as in the former case. On the fifty-fourth day the cure was complete, the uterus being retained in its proper situation. Two years after this woman died of an apoplectic attack, when the condition of the parts were examined. The vagina presented four greyish-coloured resistent lines corresponding to the incisions. The mucous membrane had acquired considerable hardness, especially that towards the orifice of the vagina, which had lost much of its ordinary dimensions. The uterus preserved its normal structure, but its volume was evidently much increased.

On the Treatment of Venereal Vegetations. By M. Vidal. (*Journal de Pharmacie et de Chimie*, September 1845.) The venereal vegetations which occur on the penis, and which so frequently resist for long the ordinary treatment, returning even after being removed by the knife or by caustics, M. Vidal has found may be successfully removed by a powder consisting of burned alum and powder of savine. These in equal parts are laid over the vegetations twice daily, and without producing any pain cause them to shrivel and disappear. The powder is applied dry, when the discharge causes it to adhere and exert its curative action.

Extract of Nux Vomica in Prolapsus Ani. By M. Barez. (*Journal de Medecine,* July 1845.) *Prolapsus ani,* so troublesome a complaint in the young after diarrhœa, &c., and so often rebellious to treatment, M. Barez says he has succeeded in easily removing by means of an aqueous solution of nux vomica. His formula is this: four scruples of salep are boiled for twenty minutes in three ounces and a-half of water, and after being strained there is added three-fourths of a grain of the aqueous extract of nux vomica. A spoonful of this mixture is given at intervals, so that the whole is taken in 24 hours.

STATISTICS.

Report on the Mortality of Edinburgh and Leith, for the months of March, April, and May 1846. By James Stark, M. D. of Edinburgh, Fellow of the Royal College of Physicians of Edinburgh.—Since the beginning of this year the mortality in Edinburgh has been unusually high. What makes this the more remarkable is the fact that last year the mortality was lower than for three previous years; and that this year there is no fatal epidemic prevalent among us to account for the increasing number of deaths. The average monthly mortality for the last six years amounted to 356; whereas the average monthly mortality of the five past months of this year amounts to 393—showing a mean increase of 37 deaths per month.

The Registrar-General's Reports put it in our power to compare the mortality here with that of London and of England since the beginning of the present year. From these we learn that London, unlike Edinburgh, has been unusually healthy this season; in fact, healthier than it has been for a period of eight years at least. It is a very curious circumstance, however, that though the same has been more or less the case with the southern and middle districts of England, the northern division has not been so highly favoured, and has more nearly resembled Edinburgh in exhibiting an increased mortality.

The low rate of mortality in London is with great justice ascribed by the Registrar-General to "the extraordinary mildness of the winter of 1846;" and the striking fact is stated that, excepting two weeks, the mean temperature since January has always been above 40°. As a natural consequence of this the range of temperature has been small. In Edinburgh, on the other hand, the winter, though open and mild, and the mean monthly temperature never below 42°, exhibited many sudden alternations of temperature, which are always noticed to be prejudicial to health. Thus, for instance, in London during March the range of the thermometer was 27° only, and in April the same; while in Edinburgh during March the range of temperature was 46°, and in April 39°. No wonder, therefore, that our mortality should be high, because, other things being equal, it is these sudden alternations of heat and cold which render one country or situation more unhealthy than another.

With the view of directing the attention more particularly to the connection of mortality with atmospheric agencies, but especially with temperature, the following table is drawn up—the mortality being limited to Edinburgh alone.

TABLE I.

Month.	Barometer.					Temperature.			
	Highest.	Lowest.	Mean.	Range.	Rain.	Highest.	Lowest.	Mean.	Range.
Jan.	30·18	28·52	29·09	1·66	2·64	59°	25°	42·47	34°
Feb.	30·04	28·56	29·43	1·48	1·60	64	28	42·31	36
March,	30·20	28·56	29·30	1·64	0·97	63	17	42·15	46
April,	30·00	28·70	29·59	1·30	2·88	64	25	44·22	39
May,	29·98	28·72	29·60	1·26	1·21	70	31	53·00	39

Month.	Mortality.					
	15 and under.	15 to 60.	Above 60.	Total deaths.	Still-born.	Grand total.
Jan.	167	136+6	73	382	27	409
Feb.	137	112+7	71	327	27	354
March,	164	158+6	73	401	33	434
April,	155	138+2	82	377	16	393
May,	158	136+2	61	356	21	377

Note.—Canaan Cottage, where the meteorological registers are kept, is situated about a mile to the south of Edinburgh, in latitude 55° 57′, and is 246 feet above the mean level of the sea.

The following tables exhibit an abstract of the mortality in Edinburgh and Leith for the months of March, April, and May, classified according to ages and diseases. It may be remarked that the numbers given include as nearly as possible the absolute amount of deaths in each place respectively; as in addition to the returns from the burying-grounds, and the medical certificates, the principal undertakers have kindly furnished me with the particulars of those cases where the bodies were removed for interment beyond the bounds of Edinburgh. All those cases brought from the country to the Royal Infirmary are excluded—the tables being limited to the residenters in Edinburgh and Leith. It is to be regretted that the classified table of diseases for Leith should be so very imperfect for the months of March and April. This resulted from the imperfections of the plan of registration adopted at South Leith, but it is to be hoped that this will not again occur, and that nothing in future will prevent the diseases there being returned as completely as for Edinburgh.

TABLE II.
EDINBURGH TABLE OF AGES.

	March.			April.			May.		
Age.	M.	F.	Tot.	M.	F.	Tot.	M.	F.	Tot.
Still-born,	23	10	33	11	5	16	17	4	21
1 year and under	45	30	75	43	35	78	42	34	76
2 years,	18	19	37	17	24	41	16	17	33
5,	18	13	31	11	7	18	16	11	27
10, .	7	7	14	6	5	11	9	8	17
15, .	4	3	7	4	3	7	2	3	5
20, .	7	4	11	9	4	13	13	9	22

	March.			April.			May.		
Ages.	M.	F.	Tot.	M.	F.	Tot.	M.	F.	Tot.
30 years,	21	15	36	11	17	28	11	23	34
40,	16	18	34	13	11	24	14	7	21
50,	27	19	46	23	20	43	14	14	28
60,	16	15	31	12	18	30	17	14	31
70,	12	22	34	16	18	34	13	8	21
80,	9	16	25	15	16	31	11	17	28
90,	4	6	10	4	10	14	1	11	12
100,	0	4	4	1	2	3	0	0	0
Not stated,	2	4	6	1	1	2	1	1	2
Total,	229	205	434	197	196	393	197	181	378

TABLE III.

EDINBURGH TABLE OF DISEASES.

Class.	Diseases.	March.	April.	May.
I.	Small-pox, (vaccinated,)	0	1	1
	do. not vaccinat.	1	2	5
	Measles,	28	22	20
	Scarlatina,	0	1	2
	Hooping cough,	13	17	18
	Croup,	8	8	3
	Thrush,	10	4	6
	Diarrhœa,	2	3	5
	Dysentery,	1	0	0
	Influenza,	1	0	0
	Remittent fever,	2	2	2
	Typhus fever,	21	16	11
	Erysipelas,	6	5	7
	Syphilis,	1	0	1
II.	Hæmorrhage	0	1	3
	Dropsy,	11	11	12
	Abscess,	1	0	0
	Mortification,	0	1	1
	Scrofula,	0	1	0
	Cancer,	3	3	1
	Debility,	16	17	15
	Sudden death,	2	2	0
III.	Cephalitis,	4	0	2
	Hydrocephalus,	22	14	19
	Apoplexy,	9	5	9
	Paralysis,	9	10	7
	Convulsions,	1	5	0
	Epilepsy,	0	1	1
	Insanity,	1	1	1
	Delirium tremens,	2	4	0
	Disease of Brain,	2	4	0
IV.	Laryngitis,	0	1	0
	Quinsey,	2	0	0
	Bronchitis,	2	3	1
	Pneumonia,	18	21	20
	Hydrothorax,	1	5	3
	Asthma,	10	6	3
	Consumption,	64	57	67
	Lung disease,	3	1	0
V.	Pericarditis,	1	0	0
	Heart disease,	6	7	5
VI.	Teething,	6	9	6
	Enteritis,	7	9	6
	Peritonitis,	3	3	1
	Tabes mesenter.	11	7	12
	Ulcer. of stomach or intestines,	1	0	2
	Hernia,	2	0	0
	Colic or ileus,	0	1	1
	Hæmatemesis,	1	0	0
	Stomach disease,	2	0	1
	Jaundice,	1	1	1
	Liver disease,	3	2	2
VII.	Ischuria,	0	0	1
	Diabetes,	1	1	0
	Stone,	1	1	0
	Kidney disease,	2	1	2
VIII.	Child-birth,	1	3	2
	Puerperal fever,	2	0	3
	Paramenia,	2	0	0
	Disease of uterus,	1	0	1
IX.	Rheumatism,	2	0	2
	Spine and joint disease,	3	3	4
X.	Skin disease,	0	0	1
	Fistula,	0	1	0
XI.	Old age,	36	45	36
XII.	Intemperance	0	2	0
	Privation,	1	0	0
	Violent deaths and suicides,	11	13	15
XIII.	Still-born,	33	16	21
	Causes not specified,	18	13	7
	Total,	434	393	378

TABLE IV.

LEITH TABLE OF AGES.

	March.			April.			May.		
Ages.	M.	F.	Tot.	M.	F.	Tot.	M.	F.	Tot.
Still-born,	4	1	5	3	0	3	2	5	7
1 year & under	7	4	11	3	3	6	11	4	15
2, .	2	1	3	2	3	5	4	2	6
5, .	0	1	1	2	1	3	3	2	5
10, .	0	1	1	1	0	1	0	0	0
15, .	1	1	2	0	0	0	0	0	0
20, .	3	0	3	0	1	1	2	0	2
30, .	4	1	5	3	3	6	3	2	5
40, .	4	2	6	2	2	4	6	5	11
50, .	2	4	6	4	1	5	2	2	4
60, .	2	1	3	3	0	3	2	5	7
70, .	4	3	7	2	2	4	2	3	5
80, .	3	4	7	4	2	6	0	5	5
90, .	0	0	0	2	0	2	0	0	0
Total,	36	24	60	31	18	49	37	35	72

TABLE V.

LEITH CLASSIFIED TABLE OF DISEASES.

Class.	Diseases.	March.	April.	May.	Class.	Diseases.	March.	April.	May.
I.	Measles, .	0	0	3	IV.	Laryngitis,	0	1	1
	Hooping cough,	2	2	6		Bronchitis,	0	0	1
	Croup, .	0	2	0		Pneumonia,	1	0	4
	Thrush, .	0	0	1		Asthma,	0	0	2
	Influenza,	0	0	1		Hydrothorax	1	1	0
	Typhus fever,	1	1	2		Consumption,	4	4	8
	Erysipelas,	0	1	1	V.	Heart disease,	0	3	3
II.	Dropsy, .	0	1	2	VI.	Dis. of diges. org.	2	6	7
	Debility, .	3	0	1	VII.	Stone, .	1	0	0
	Other diseases of uncertain seat,	0	0	4	VIII.	Child-birth,	1	0	1
III.	Cephalitis, (meningitis,)	0	1	2		Puerperal fever,	0	0	1
	Hydrocephalus,	0	1	2	XI.	Old age,	6	7	5
	Paralysis,	0	3	2	XII.	Violent deaths,	0	1	2
	Convulsions,	0	0	1	XIII.	Still-born,	5	3	7
	Epilepsy,	0	0	1	Not specified,		31	10	0
	Disease of brain,	0	1	1		Total,	60	49	72

From the above classified table of diseases it will be seen, that, with the exception of the usual children's diseases, measles and hooping-cough, there is no epidemic disease among us to account for the increased mortality of the present year. Typhus fever, which in the earlier months attacked persons of all classes, but was not by any means very prevalent, has during the last month materially declined, and the fatal cases have been confined to the lower and middle classes. Erysipelas alone has been somewhat more prevalent.

Small-pox, from which Edinburgh has been unusually free this year, has during the last month made its appearance in several localities, and proved fatal in six instances. As usual, it has chiefly attacked those who were not vaccinated.

Only one of the fatal cases during March was reported to be vaccinated, but on making particular inquiry I have not been able to get satisfactory proof that such was the case. The fatal vaccinated case in April was twice successfully vaccinated, yet took unmodified small-pox and died under it. Even though such a person had taken small-pox itself and recovered, he would, in all likelihood, have been as liable as ever to a second attack. Two such instances I have witnessed in my own practice, in which children, after successful vaccination, took unmodified small-pox on being exposed to its contagion; and after the lapse of about a year were again successfully vaccinated, yet were seized with unmodified small-pox a second time, from being taken to a house in which lay a small-pox patient. My own conviction is, that these persons remain as liable to a third attack as if they had never been vaccinated, or had never taken small-pox.

The fourth class of diseases, those of the respiratory organs, are those which over Europe cut off the greatest proportional number of persons, very nearly an exact third of the mortality belonging to that class. As might be expected, these diseases are somewhat less prevalent in one locality than in another; but it does not seem to follow, that the less the mortality is in proportion to the population; the less should be the proportion of deaths from diseases of the respiratory organs. London and Edinburgh are very good examples of the general truth of this remark. In London during the past months of this year, the mortality has amounted to only 1 in 103 living. In Edinburgh during the same period, the mortality, (excluding the still-births,) has been 1 in 76, or between a fourth and a fifth higher than in London for the same period. In London, however, the deaths from diseases of the respiratory organs, including hooping-cough and croup, were in the proportion of 376 to every 1000 deaths; whereas in Edinburgh, the same diseases were to the 1000 deaths in the low proportion of 292, showing a difference in favour of the superior salubrity of Edinburgh to the extent of 84 out of the 1000 deaths, and this too in a year when the mortality is above the average.

The safest way, however, to estimate the proportional mortality of diseases is to compare their proportion both with the population and with the total deaths. Estimated in this way, London has lost from diseases of the respiratory organs, including hooping-cough and croup, 1 in every 273 of its present population, or, as above stated, in the proportion of 376 out of every 1000 deaths. Edinburgh, during the same period, has lost 1 in every 260 of its present population from these diseases, or in the proportion of 292 out of the 1000 deaths. If the deaths from consumption alone be estimated in the same way, it is found that in London 1 has died out of every 733 living, or in the proportion of 140 out of the 1000 deaths, whereas Edinburgh has lost from the same cause 1 out of every 501 living, or in the proportion of 150 out of every 1000 deaths.

In my first report I was led into an error as to the still-births being registered in England, in consequence of the Statistical Nosology, stating that premature births were registered under the head of debility, and in consequence of an explanatory note being appended, which quoted a passage from Dr Granville's work referring exclusively to still-births. I am, however, informed by the Registrar-General, that still-births are not registered in the English tables of mortality. This appears to me to be an important oversight, because in questions affecting the condition and health of the inhabitants, many important conclusions may be drawn from an accurate statement of the particulars connected with still-births. M. Lugol, in his Researches on Scrofula, attributes the mortality of the *fœtus in utero* to the existence of the scrofulous constitution in one or both parents, and unless means are used to register such facts both in town and country, no data can be procured for the confirmation or refutation of such statements.

21 *Heriot Row, June* 1846.

THE

EDINBURGH
MEDICAL AND SURGICAL JOURNAL.

No. CLXIX.

CONTENTS.

PART I.—ORIGINAL COMMUNICATIONS.

ART. I. Cases of Partial Aneurism of the Heart, with Remarks. By Thomas Bevill Peacock, M.D., Physician to the Royal Free Hospital, and to the Royal General Dispensary, London, Page 261

II. On the Age of Puberty in the Island of Madeira. By John Roberton, Esq. Manchester, . . 281

III. Case of Phosphorescence in the Human Body. By Henry M'Cormack, M.D., . . . 285

IV. The Power of the Mind over the Body: an Experimental Inquiry into the nature and cause of the Phenomena attributed by Baron Reichenbach and others to a "New Imponderable." By James Braid, M.R.C.S.E., C.M.W.S., &c. . 286

V. An Experimental Inquiry into the Functions of the Ophthalmic Ganglion; with an application of the inferences obtained to the physiology of the Ganglionic System in general. By C. Radclyffe Hall, M. D. . . . 312

VI. Surgical Cases, with Observations. By Dr David Kerr, 353

VII. Observations on the Report lately published by the Health of Towns' Commission, and on the remedial measures therein proposed for England and Wales. By Richard White Young, M. D. . . . 358

VIII. Termination of the case of Mary Riordan, published in Vols. IV. and V. of Transactions of College of Physicians in Ireland. By William Pickells, A. B., M. D. . . 384

IX. Fatal Case of Military Flogging at Hounslow, . 395

PART II.—CRITICAL ANALYSIS.

ART. I. The Vital Statistics of Glasgow for 1843 and 1844. Drawn up by appointment and under the authority of the Lord Provost, Magistrates, and Town-Council. By Alexander Watt, LL.D. 434

II. Clinical Illustrations of the Diseases of India, as exhibited in the Medical History of a body of European Soldiers for a series of years from their arrival in that country. By William Geddes, M. D., ... 446
III. The Sanative Influence of Climate. By Sir James Clark, Bart. M. D. 475
IV. 1. The Baths of Germany, considered with reference to their remedial efficacy in Chronic Diseases, with an Appendix on the Cold Water Cure. By Edwin Lee, Esq.
2. Practical Observatious on Mineral Waters and Baths, with Notices of some Continental Climates, and a Report (the Third) of the Cold Water Cure. By Edwin Lee, Esq.
3. A Description of the Mineral Springs of Aix-la-Chapelle and Borcette. By L. Wetzlar, M. D. . . 481

PART III.—Medical Intelligence.

On Food and its Principles, . . . 494
Bread, on the preparation of, ... 500
Case of Partial Aneurism of the left Ventricle of the Heart, 603
Opium not poisonous to the Rabbit, . . 512
Case of poisoning by Carbonate of Lead, . . . ib.
Action for Damage to Vegetation by Muriatic Acid Gas, . ib.
Report of the Committee on Quarantine Regulations read to Annual General Meeting held 4th May 1846, ... 515

Errata in Dr Stratton's paper for July 1846.

Page 74, line 27, *for* " 1834" and " 1833," *read* " 1833 and " 1832."
Page 77, line 24, *for* " checked by quotidian," *read* " checked quotidian."

NOTICES TO CORRESPONDENTS.

Communications have been received from Mr Hall, Mr Wilson, and Mr Robinson.

The following works have been received :—

Commentary on the Hindu System of Medicine. By T. A. Wise, M.D.

On the Use of the Body in Relation to the Mind. By George Moore, M. D., Member of the Royal College of Physicians, &c. London, 1846.

Dr Hooper's Physicians' Vade Mecum ; or Manual of the Principles and Practice of Physic. New edition, considerably enlarged and improved, with an Outline of General Pathology and Therapeuties. By William A. Guy, M. B., Cantab., F. R. C. of Physicians, &c. &c.

Deontologie Medicale, ou Des Devoirs et des Droits des Medecins dans l'Etat Actuel de la Civilization. Par le Docteur Max Simon.

Ueber Die Brighitsche Nierenkrankheit, Eine Akademische Abhandlung von Peter H. Malmsten der Medecin und Chirurgie Doctor. Aus dem Schwedischen ubersetzt und mit einigen Anmerkungen versehen von Gerhard von dem Busch, Doctor Der Medicin und Chirurgie u. s. w.

Observations and Essays on the Statistics of Insanity ; including an Inquiry into the Causes influencing the results of treatment in Establishments for the Insane ; to which are added the Statistics of the Retreat near York. By John Thurnam.

Kosmos : Sketch of a Physical Description of the Universe, by Alexander Von Humboldt, vol. I. Translated under the Superintendence of Lieut.-Col. Edward Sabine, R. A., For. Sec. R. S. London, 1846.

An Easy Introduction to Chemistry. By George Sparkes, late Madras Civil Service. Second Edition. London, 1846. 12mo. Pp. 182.

The Water Cure in Chronic Diseases ; an Exposition of the Causes, Progress, and Terminations of various Chronic Diseases in the Digestive Organs, Lungs, Nerves, Limbs, and Skin. And of their Treatment by Water and other Hygienic Means. By James Manby Gully, M. D., Licentiate of the Royal College of Physicians. London, 1846. Post 8vo.

A Practical Treatise on the Diseases of Children. By James Milman Coley, M. D., Member of Royal College of Physicians in London, &c.

The Brain and its Physiology ; a Critical Disquisition on the Methods of determining the relations subsisting between the Structure and Functions of the Encephalon. By Daniel Noble.

Clinical Collections and Observations on Surgery, made during an attendance on the Surgical Practice of St Bartholomew Hospital. By W. P. Ormerod, Fellow of the Royal College of Surgeons of England, &c. London, 1846. 8vo. Pp. 512.

The Why and the Wherefore ; or the Philosophy of Life, Health, and Disease. New and Original Views, explanatory of their Nature, Causes, and Connection, and of the Treatment of Diseases upon a few General Principles, based upon the laws of Nature and Common Sense : with Rules for the preservation of Health and Renovation of the System. The Fruit of Thirty years' Observation and Professional Experience. By Charles Searle, M. D., M. R. C. S. E., and late of E. I. C. Madras Establishment. London, 1846. 8vo. Pp. 206.

Fever Physiologically considered ; Considerations on Yellow Fever, Typhus Fever, Plague, Cholera, and Sea Scurvy ; also the Questions of Contagion and the Quarantine Laws ; with an Address to the Public on the Popular Treatment of Cholera. By David M'Connell Reed, Esq. Licentiate of Medicine, Member of the Royal College of Surgeons of Edinburgh, &c. &c. London, 1846. Post 8vo. Pp. 263.

A Guide to the Buxton Waters. By William Henry Robertson, M. D., &c. Third Edition, revised. London, 1846. 12mo. Pp. 31.

On Wounds and Injuries of the Arteries of the Human Body ; with the Treatment and Operations required for their Cure. Illustrated by 130 Cases, &c. By G. J. Guthrie, F. R. S. London, 1846. Large 8vo. Pp. 97.

Hints for Pedestrians. By Medicus. With Illustrations. London, 1846. Second Edition. Square 12mo. Pp. 110.

A System of Surgery. By J. M. Chelius, Doctor in Medicine and Surgery, &c. Translated from the German, and accompanied with Additional Notes and Observations. By John. F. South, Professor of Surgery, &c. London, 1846. Part XII. P. 321—480. Part XIII., 481—640.

Observations on the Edinburgh Pharmacopœia and on the Dispensatories of Dr Christison and Dr A. T. Thomson, &c. By Richard Phillips, F. R. S. L. and E., &c. London, 1846. 8vo. Pp. 92.

Notes on Epidemic Cholera. By R. Harkley Kennedy, M. D., &c. Second Edition revised. London, 1846. Post 8vo. Pp. 239.

On the Antidotal Treatment of Epidemic Cholera. By John Parkin, M. D., &c. London, 1846. 8vo. Pp. 60.

Thirty-second Annual Report of the Directors of the Glasgow Royal Asylum for Lunatics. Submitted, &c. 8th January 1846, with the Fifth Report of the Physician. Gartnavel, 1846. 8vo. Pp. 44.

Report of the Directors of the Montrose Lunatic Asylum, Infirmary, and Dispensary. Instituted 1781. For the year ending 1st June 1846. Montrose, 1846. 8vo. Pp. 22.

Medical Report of the House of Recovery and Fever Hospital, Cork Street, Dublin, for two years; from 1st January 1844 to 31st December 1845. By George A. Kennedy, M. D., M. R. I. A., &c. Dublin, 1846. 8vo. Pp. 59.

Some Account of an Epidemic Fever which prevailed in Liverpool in the latter months of 1844. By George Churchhill Watson, M. D., &c. Worcester, 1846. Large 8vo. Pp. 26.

The Microscopic Anatomy of the Human Body in Health and Disease. Illustrated with numerous drawings in colour. By Arthur Hill Hassall, Member of R. C. of Surg. London, &c. London, 1846. 8vo. Part I, August, pp. 38. Part II, September, pp. 36.

The Blood Anatomically, Physiologically, and Pathologically considered. The Warneford Prize Essay for 1845. By Charles Edward Joseph, M. R. C. S., London. Birmingham, 1846. 8vo. Pp. 126.

The Liverpool Health of Towns' Advocate. Part I. Edited by John Sutherland, M. D., &c. Liverpool, 1846. 8vo. Pp. 124.

The Retrospect of Medicine. Being a Half-Yearly Journal containing a Retrospective view of every Discovery and Practical Improvement in Medicine. Edited by W. Braithwaite. Vol. XIII. January,—June, 1846. London, 1846. 12mo. Pp. 504.

The Half-Yearly Abstract of the Medical Sciences; a Practical and Analytical Digest of the Contents of British and Continental Medical Works published in the preceding six months. Edited by W. H. Ranking, M.D., Cantab. Vol. III. January—June, 1846. London, 1846 Post 8vo. Pp. 409.

The Medico-Chirurgical Review, and Journal of Practical Medicine. New Series. No. 7, 89 of old. July 1846. London. 8vo.

The British and Foreign Medical Review, or Quarterly Journal of Practical Medicine and Surgery. Edited by John Forbes, M. D. &c. No. 43. July 1846. London. 8vo.

Transactions of the Medical and Physical Society of Bombay for 1844. No. 7. Bombay, 1844.

The American Journal of the Medical Sciences. Edited by Isaac Hay, M. D. No. 23. New Series. July 1846. Philadelphia.

The London Medical Gazette, Session 1845, 1846. No. 59. June 12 or 967—No. 73 or 981. September 18, 1846.

The Medical Examiner and Record of Medical Science. Edited by Robert M. Huston, M. D. No. 13. January 1846, No. 16; February, No. 15, March; No. April; No. 16, 17, May. Philadelphia, 1846.

Weekly Tables of Mortality for the Metropolis. No. 24, Saturday, June 13, 1846; No. 37, Saturday, 12th September 1846.

THE

EDINBURGH MEDICAL AND SURGICAL JOURNAL.

1. OCTOBER 1846.

PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*Cases of Partial Aneurism of the Heart, with remarks.* By THOMAS BEVILL PEACOCK, M. D., Physician to the Royal Free Hospital, and to the Royal General Dispensary, London.

THE first published case of partial dilatation or aneurism of the heart, in which the affection was fully understood and described, is that recorded by Professor Walter of Berlin* in 1785. It appears, however, that the disease was known to John Hunter prior to the publication of Professor Walter's memoir, Dr Thurnam† having found in the Hunterian MSS., in the possession of the Royal College of Surgeons, a full description of an aneurism of the apex of the heart, in the case of General Herbert, who died in 1757; together with notes of two other cases, of which the preparations exist in the museum, affording examples of the incipient disease. In 1757 a case of rupture of the left ventricle was also published by Galeatti,‡ which may probably have been connected with aneurism.

Baillie,§ in the first edition of his Morbid Anatomy, published in 1793, referred to the occasional occurrence of dilated pouches in the walls of the heart, and has figured the appearances in the only instance which had fallen under his notice; and Corvisart[*] described in 1806, the particulars of a case of the kind which occurred to him in 1796. In 1827 Breschet,[†] in a special memoir on this disease, to which he has applied the term of false consecutive aneurism of the heart, collated, in addition to the cases of Walter, Baillie, and Corvisart, others which had occurred to Zanini, Rostan, Cruveilhier, Dance, and the two Berards, together with that of the celebrated Talma, and the description of a specimen of the disease preserved in the museum of the Faculty of Medicine of Paris. Subsequently instances of this form of cardiac lesion have been related by Reynaud,[‡] Petigny,[§] Cruveilhier,[‖] Bignardi,[¶] Sir A. Cooper,[**] Adams and Harrison,[††] Elliotson,[‡‡] Hope[§§] and others; and in 1838 my friend Dr Thurnam, in an elaborate memoir communicated to the Medico-Chirurgical Society of London, related the histories of eight cases not previously published, and entered into a numerical analysis of these and of several other less perfect cases which had fallen under his notice, as well as of the whole of those which had previously been recorded. Since that period several interesting observations relative to the cardiac aneurisms have appeared, among which the memoir of Dr Craigie, in the 59th volume of this Journal, the case of Dr Pereira, in the Medical Gazette for October 10, 1845, and the article of Rokitansky, in his "Handbuch der Pathologischen Anatomie," may be specially referred to.[‖‖]

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* Nouveaux Memoires de l'Academie de Berlin, 1787, p. 57.

† On aneurisms of the heart, with cases, Med. Chir. Trans. vol. xxi. 1838.

‡ Comment. de Bonon. Academia :—opuscula, p. 16, T. iv.

§ Morbid Anatomy of the Human Body, p. 17.

I.—*Partial Aneurism of the Left Ventricle.*

In the following communication I purpose giving the particulars of a case of partial aneurism of the apex of the ventricle, of a case of aneurismal dilatation of the sinus of the left auricle, and of a case of abscess in the substance of the heart; together with remarks illustrative of the modes of origin of the true and false partial aneurisms of the heart.

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[*] Maladies du Cœur. Paris, 1806. Obs. 46, p. 267.

[†] Sur l'Aneurism faux consecutif. Paris, 1827.

[‡] Journ. Hebd. de Méd. T. ii. 1829, p. 363.

[§] Journ. Univ. et Hebd. T. iii. p. 311.

[‖] Anat. Pathol. Liv. 21 and 22.

[¶] Ann. Univers. 1829. Bouillaud sur les Malad. du Cœur, T. ii. p. 207.

[**] Lect. by Tyrrell, vol. ii. p. 27. 1827.

[††] Dublin Hosp. Rep. vol. iv. 1827, p. 407.

[‡‡] Lumelyan Lect. 1830.

[§§] Morbid Anatomy. 1834.

[‖‖] For the literature of this subject see the Appendix to Dr Thurnam's memoir in the Med. Chir. Trans. vol. xxi.

The article Anéurism vrai du Cœur, by Ollivier, in the Dict. de Médécine, T. viii. p. 303.

Dr Craigie's memoir in Edinburgh Medical and Surgical Journal, vol. lix. p. 356. This author has given an abstract of the cases contained in Breschet's memoir, and notices of others more recently published, together with an extremely interesting one which had occurred to himself.

Case 1*st.*—Chronic affection of the liver; copious effusion of serum into the cavity of the right pleura; partial aneurism of the false consecutive form at the apex of the left ventricle; rupture of the sac and escape of blood into the cavity of the pericardium; death from engorgement of the left lung five days after the occurrence of the rupture.

The gentleman who was the subject of this case first came under my charge in March 1845. At this time he was 31 years of age, had been from an early period engaged as a commercial traveller, and was of very irregular habits. He was active, had always enjoyed remarkably robust health, and never suffered from any rheumatic affection, with the exception of feeling, occasionally during winter, slight pains in the arms. The history which he gave of the illness under which he laboured, was as follows:—

In the early part of the January previous, while on a journey, he was suddenly seized one afternoon at Nottingham, with pain in the head, dizziness, and sickness, which, however, soon subsided. A fortnight elapsed, during which time, though far from well, he continued his journey, but on reaching Manchester he was compelled to obtain medical assistance. At this time he suffered from a feeling of nausea and sickness, pain in the abdomen, and tenderness, more especially in the right hypochondrium and epigastrium. He took some medicine, and had leeches and blister applied to the right side, from which he obtained relief, but not feeling equal to his engagements he came to town.

I first saw him on the 17th March. He then complained of nausea and occasional attacks of sickness, with pain across the pit of the stomach; he was slightly jaundiced; the pulse was somewhat increased in frequency and feeble; the tongue pale at the tip, furred and brown towards the centre and base. The abdomen was tumid and tender over the whole upper portion, more especially above the umbilicus. The ankles swelled somewhat towards the evening; the stools were clay-coloured, and the urine much darker than usual, depositing a copious sediment of the lithates. It was not albuminous. The upper part of the chest on both sides sounded clear on percussion; but on the right side, the hepatic dulness extended to a higher level than usual, and the liver could be felt distinctly below the borders of the ribs and extending across the *scrobiculus cordis.* The respiration was naturally performed, and auscultation of the sounds of the heart afforded nothing unusual.

He was directed to take small doses of calomel, and to have a few leeches applied at intervals to the epigastrium and right hypochondrium, to be followed by blisters and counter-irritants, with mild aperients and a strictly regulated diet. Under this treatment he experienced considerable improvement. The yellow tinge of the conjunctivæ almost disappeared; the feces became more bilious, and the urine paler. The nausea, tenderness, and swelling of the abdomen, and the œdema of the ankles nearly left him; his tongue became clearer, though still pale; and his appetite improved. On seeing him on the 17th April, I found him much altered for the worse. A few days before, he had been seized with pain in the right side, difficulty of breathing and feverishness. For these symptoms he had applied leeches, and had obtained some relief. His pulse was quicker than it had been, and more feeble; he still complained of pain in the right side, and had difficulty of breathing, more especially on exertion. On examining his chest, I found decided signs of what had been before suspected, the presence of fluid in the right pleural sac. The chest sounded dull on percussion as high as the level of the nipple, and beneath this point there was an entire absence of the respiratory sounds and of the vocal thrill, while bronchial respiration was heard near the nipple. The upper part of the right side, and the whole of the left, sounded clear on percussion, and the respiratory murmur was in these situations more active than usual. The lower part of the right side was obviously enlarged, and measured about one-fourth of an inch more in girth than the left. The liver was pressed downwards and to the left side, so as to render the right hypochondrium and epigastrium prominent. He was compelled to lie on the right side.

As the state of his general health did not allow of any active depletion, he was directed to apply a blister, and to take mild alteratives, with diuretic and saline medicines. After a few days, the mouth having become slightly sore, preparations of iodine were given. He was allowed a more nutritious diet with a small quantity of porter, and he went to the sea side. After his return on the 4th May he thought himself better for the change of air; his appetite was tolerably good, and he complained chiefly of debility; his pulse, though regular, was extremely feeble; he experienced considerable difficulty of breathing on exertion or exposure to cold; and his face was somewhat livid. I did not see him again till the 14th of May, when I was requested to visit him at his residence, and found him dying. He was breathing with the greatest difficulty and incapable of lying down. The face, hands, and feet, were cold and livid; the surface of the body covered with a cold sweat, and the pulse at the wrist so feeble as to be scarcely perceptible. His intelligence was not, however, impaired,—he had no cough or expectoration, and complained only of the agonising difficulty of breathing. On the right side, the chest sounded dull over the greatest portion of its extent, and copious crepitant and subcrepitant râles were heard in the subscapular region. From the position which he occupied, it was impossible to ascertain the state of the left side of the chest in front; but behind, it sounded relatively clear; in the subscapular region the respiration was bronchial.

He died at a quarter to eight o'clock on the evening of the same day.

I found on inquiry that no particular change had been observed before the 9th, and that he had as usual walked out on the previous day. On the 9th he suddenly complained of feeling a sensation, as if something had burst or given way in the region of the heart, and he immediately became sick and faint. These symptoms, however, subsided in a short time, and for the two following days he was much as usual. On the 12th, be begun to experience increased difficulty of breathing, and on the 13th his respiration became extremely laborious. From this time he rapidly sank into the state in which I found him.

In the examination of the body I had the kind assistance of Mr Haynes of Holloway, who had been called to him during the last hours of his life. The examination took place at 2 P. M. on the 15th May, 18 hours after death.

Autopsy.—There existed a moderate degree of emaciation; the face was still very livid, and the expression of countenance anxious; the abdomen and legs were not swollen.

The right side of the chest contained a large amount of fluid (between two and three quarts), of a pale yellow colour, and tolerably limpid, though containing loose flocculent portions of lymph. The right lung was much compressed and solid, except at the apex, where a small portion was still sparingly crepitant. It adhered below by several old, and very much extended bands, to the central portion of the diaphragm; and closely to the ribs in the infra-scapular region. The pleura was generally opaque, and on the diaphragm displayed two masses of lymph, about the size of five shilling pieces, and had an appearance as if of ulceration. The right lung was found on section to be of a deep purple colour. The left lung was throughout adherent by firm and old attachments, but was very voluminous. In front it was tolerably crepitant; but at the posterior part of the upper lobe, and partially also of the lower, it was entirely solid, and on compression exuded thin serum. The bronchial tubes in both lungs contained much secretion, and the mucous membrane was of a deep purple colour.

The heart and pericardium occupied a very large portion of the chest, the base extending to a higher level than usual, while the apex reached to the cartilage of the seventh rib.

The abdominal cavity contained a small quantity of pale yellow-coloured serum. The liver had evidently been pushed downwards, and towards the left side, by the large accumulation of fluid in the right pleural sac. It was of unusually large size, the enlargement occupying more especially the left lobe; its form was somewhat irregular, and the serous coat was, in several places, opaque and much thickened, and the superficial venous trunks were greatly dilated, as were also those of the under surface of the diaphragm. On section the organ was found intensely engorged, and the exposed surface was dense and granular. The gall-bladder contained healthy bile, and the ducts were free from obstruction. The spleen was very large, solid, and of a deep purple colour; it adhered to the parietes of the abdomen, and its convex surface displayed several firm and thick cartilaginous plates. The kidneys were large, hard, and much engorged, but were otherwise free from disease.

The mucous membrane of the stomach was of a brownish colour, and marked by redness from imbibition in the course of the vessels; it was thick and very lacerable, not admitting of being drawn off in streaks; at the pyloric extremity it was somewhat granular. The intestines were not opened, but presented externally no other unusual appearance than intense venous congestion.

On careful examination the pericardium was found greatly thickened, and adhered to the heart over its entire surface, with the exception of a portion of the apex, where there existed a small quantity of bloody coloured fluid. The attachments between the reflected and visceral pericardium were very soft, and readily admitted of being torn through over the inferior portion of the organ; but at the base, more especially behind, the two membranes were only capable of being separated by dissection with the knife.

On the surface of the heart being exposed, it was found to be covered with thick masses of soft fibrine, partially decolorized, and these seemed to be the result of blood forced between the layers of the pericardium, which had previously been adherent. The form of the heart was very irregular, its apex being much prolonged and separated from the rest of the organ by a furrow, broad and shallow in front, but deep and very distinct behind. The organ thus resembled a somewhat enlarged heart, with a tumour, the size of a small fist, appended to its apex, and projecting posteriorly. On a section being made into the left ventricle, commencing in front near the base and carried to its lower extremity, the enlargement of the organ was found to be owing to an aneurismal dilatation of its apex. The cavity of the left ventricle was nearly of its ordinary dimensions, and its parietes were not much hypertrophied, measuring only 5¼ French lines* at its thickest part, and being nearly of the same width from the base to the origin of the aneurismal sac. The muscular substance, except in the immediate neighbourhood of the sac, retained its natural colour and consistence. The sac commenced somewhat abruptly one inch and ten lines from the attached margins of the aortic valves, and its orifice measured three inches in circumference. At this point the walls of the ventricle were somewhat thicker than elsewhere, and the muscular substance was paler and firmer, being intermixed with pale tendinous fibres. The endocardium was thickened and opaque. Immediately beyond the orifice of the sac, its cavity expanded to five inches and nine lines in circumference, and the walls of the ventricle ceased abruptly at this point. The boundary of the sac was first formed by the thickened endocardium and pericardium, and nearer towards its apex by the pericardium alone. The latter membrane over the entire surface of the heart was from one line to one line and a half in thickness. Where it bounded the base of the aneurismal sac it was not less than two to two and a half lines thick, and was peculiarly dense and firm. From this point its thickness again decreased, and at the apex it was so thin as to be scarcely traccable, being intimately blended with the laminated coagula contained in the cavity. The sac of the aneurism, having been prevented increasing in depth by the attachment of the pericardium to the diaphragm, was broader from side to side and from before backwards than from above downwards. In capacity it would have contained a large hen's egg. The base of the sac was covered by distinct laminæ of fibrine, the outer of which were decolorized, and so firmly attached to the boundary of the sac at the apex, as to be scarcely separable, while towards the cavity of the aneurism they had the usual character of recent coagula. No distinct laceration of the sac was found, the blood having escaped by a general giving way of the parietes. At the neck of the sac behind, the altered muscular tissue ceased abruptly; while, in front, the cavity of the ventricle and that of the aneurism presented a less distinct line of separation. The *columnæ carneæ* and the inner layers of the ventricular walls had extensively undergone the transformation into a pale fibrous tissue, and the endocardium was opaque, white and thickened. The aortic valves were healthy, only presenting some slight opacity. The mitral valve opaque and thickened, and the cords and muscular columns stretched; the valve was, however, competent. The ascending aorta was somewhat large and its coats unusually thick, the inner membrane being in places elevated by small patches of atheroma. The orifices of the coronary arteries retained their natural size. The walls of the right ventricle were much hypertrophied, measuring throughout three lines in thickness. The pressure of the aneurismal sac had, as it were, pushed up the lower extremity of the ventricle so as materially to alter its form;

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* The measures employed throughout the paper are French inches and lines.

and the orifice of the large coronary vein in the right auricle, had probably been compressed. The tricuspid valve was healthy. The auricles were of their ordinary size. The *foramen ovale* was open for a space admitting the passage of the fore-finger, and the valvular fold closing the remainder of the aperture was pierced by several small foramina.

The following were the dimensions of the cardiac orifices, as ascertained by graduated balls.*

The aortic aperture	3 French inches and	9 lines.
The left auriculo-ventricular	4	3
The pulmonic	4	—
The right auriculo-ventricular	4	9

In the above case the existence of the large amount of fluid in the right pleural sac, and the evidences of old disease of the liver, fully confirmed the opinion which had been formed during life; its most interesting feature, however, the lesion of the heart, was wholly unsuspected before the examination. At the time the case first came under my notice, an investigation of the state of the heart excited no suspicion of its being diseased; and, indeed, an attentive examination of all the circumstances of the case renders it probable, that at this time (two months before death,) the cardiac affection was in a very early stage. Notwithstanding the increase in the total size of the heart caused by the aneurismal sac at its apex, the left ventricle was not of much greater capacity and thickness than usual, as, had the aneurism been of long duration, it must almost necessarily have been. It seems further probable that the formation of an aneurismal sac in a portion of the walls of the left ventricle, when it does not interfere with the functions of the valves, will, so long as the cavity is only of small size, be productive of no very decided symptoms of cardiac disease; an inference which is confirmed by the fact that several of the cases of this affection which have been recorded excited scarcely any symptoms attracting attention during life. When my patient was first examined, his pulse, though somewhat quick and feeble, was perfectly regular, and continued throughout the progress of the case free from irregularity though extremely feeble. He made no complaint of pain, or feeling of uneasiness in the region of the heart, and uniformly referred his sufferings to the epigastrium and right hypochondrium; and the evidences of portal obstruction seemed sufficiently to explain the swelling of the abdomen and ankles. At a later period the breathlessness on exertion, and the lividity of the face on exposure to cold, showed a very embarrassed state of the circulation; and it is probable that, had the attention not been too exclusively directed to the large effusion of fluid in the pleura, evidences would have been afforded on attentive examination of the existence of some form of cardiac disease. It seems, however, very doubtful whether, under any circumstances, a correct opinion could have been formed during life, as to the nature of the lesion affecting the heart. It must further be observed, that, as the *foramen ovale* was very largely open, it would not be safe to ascribe the lividity of the face, which is certainly not ordinarily attendant on empyema, to the aneurism at the apex of the heart.

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* I have for some time been in the practice of ascertaining the dimensions of the orifices of the heart by graduated balls. The suggestion was first made to me by the late Professor Graham of Edinburgh, and I am satisfied that it forms a much more correct means of ascertaining the relative dimensions of the orifices than the ordinary method of measuring them after they are laid open.

The appearances detected on examination of the body illustrate very clearly the cause of the sudden occurrence of the symptoms which preceded death, and of their course to the fatal termination. On the 9th, it will be remembered, that, while in his usual state of health, and capable of walking a considerable distance, without much inconvenience, he suddenly experienced a sensation, as if something had given way in the region of the heart, and felt sick and faint. At this time, doubtless, the partial rupture of the walls of the aneurism, which led to the extravasation of the blood into the cavity of the pericardium, took place. The firm adhesions of the two layers of the pericardium over the whole surface of the heart, which it is believed then existed, not admitting of any rapid escape of blood, the sickness and faintness first induced soon subsided; and for the next two days, though suffering from difficulty of breathing consequent on the more embarrassed state of the heart, he did not regard himself as materially worse. The escape of blood, however, slowly went on, as the pericardiac adhesions were torn up; the action of the heart became more and more laborious; the circulation of the left lung, which was alone permeable to air, became much impeded, and its tissue engorged and hepatized, and he then rapidly sank, dying on the fifth day from the period at which it is conceived the rupture of the sac first occurred.

Owing to the early adhesion of the reflected and visceral pericardium over the space occupied by the sacs in the lateral aneurisms of the heart, the rupture of their walls and the escape of blood into the cavity of the pericardium, which occurred in this case, would appear to be comparatively unusual results. Dr Thurnam was only able to find seven cases in which the rupture of the sac took place, out of twenty-four in which he ascertained the mode of death; and I am unable to refer to any more recent example of a similar kind. In six of these cases the blood escaped into the cavity of the pericardium, and in the seventh into that of the left pleura. In an eighth case, related by M. Rostan, death also took place from internal hemorrhage, the wall of the left ventricle adjacent to the dilated portion having given way. In all these cases death ensued immediately on the occurrence of the rupture of the sac; while in the case described in the paper, the patient survived this event five days, dying at that period, not from the internal hemorrhage, but from the obstruction to the pulmonary circulation induced.

The history of the early symptoms in this case is unfortunately so defective as to involve the cause of the cardiac disease in much obscurity. The great thickening of the reflected and attached pericardia, and their firm and intimate adhesion, not only in the vicinity of the aneurismal portion of the ventricle, but also at the base of the heart and around the origins of the large vessels, I am inclined to regard as indicative of the existence of general inflammation of the pericardium, at a period anterior to the formation of the aneurism, rather than as resulting from the adhesive process excited by the pressure of the sac. To this inflammation I am further disposed to ascribe the changes which led to the production of the aneurism; regarding them as resulting from the extension of the external disease to the muscular substance of the ventricle, either directly or by the supervention of endocarditis. In either case the walls of the ventricle being partially softened or eroded, would be capable of yielding, and of being expanded into a sac.

Pericarditis is, however, rarely an idiopathic affection. Were the inference which is here drawn, therefore, correct, there would probably exist either in the history of the patient during life, or in the appearances detected in the body after death, evidences of those affections with which it is usually combined. On inquiry, I cannot ascertain that my patient had ever laboured under any serious rheumatic affection, nor did there exist any evidence of renal disease. The two most frequent causes of pericarditis being, therefore, here absent, it becomes a question how far we may ascribe the inflammation of the pericardium to the affection of the left pleura, of which we have proof in the universal attachment of the lung to the parietes of the chest, by adhesions of a date certainly not more recent than those of the pericardium.

In the absence of any history of severe illness prior to the attack in January, it is possible that the subacute hepatic affection under which he then appears to have laboured, may have been combined with inflammation of the left pleura, on which the inflammation of the pericardium may have been superinduced. If the date here assigned for the changes which laid the foundation of the aneurismal disease be correct, its duration may be stated at between four and five months. This period may appear too short for the disease to have attained so advanced a stage as has been described. It must, however, be borne in mind, that, for the last four or five weeks, the embarrassed state of the pulmonary circulation, consequent on the compression of the right lung, must have greatly impeded the functions of the heart and have tended to the rapid expansion of the sac. It is further probable that the aneurism had increased considerably in size during the few days which preceded death.

The form of disease in this case may be referred to that of the false consecutive aneurism, of which, indeed, the sac, bounded over a large portion of its extent by the thickened pericardium alone, forms a very characteristic example. The advanced stage of the disease renders it, however, impossible to speculate as to the probable origin of the sac in dilatation of the parietes, or in the rupture or ulceration of the inner portion of the ventricular walls.

II.—*Modes of development of the Cardiac Aneurisms.*

Breschet, as the term which he employed to designate this form of disease indicates, regarded the partial aneurisms of the heart as originating in the rupture of the endocardium and a portion of the muscular substance of the ventricular walls, or more doubtfully in their erosion as the result of inflammation. Reynaud, on the contrary, showed that in the case which he described, the sac was formed by dilatation of the parietes of the ventricle connected with an altered condition of the endocardium, and was disposed to ascribe the production of the partial aneurisms of the heart to a similar process in all cases. Cruveilhier, while he admits the occurrence of the false form of aneurism, maintains that most generally the sacs have their origin in dilatation, and that the destruction of the endocardium and adjacent layers of the muscular substance which obtains in the aneurismal sacs of larger size, is the result of their gradual expansion. The first step in the production of the cardiac aneurisms he ascribed either to softening or loss of resistance in the ventricular parietes resulting from inflammation; or, more generally, to the transformation of a portion of the muscular substance into a fibrous tissue, in accordance with a general law, that portions of the parietes of the ventricles subjected to undue distension have the contractile fibres replaced by inelastic tissue. Bouillaud, on the contrary, regards the cardiac aneurisms as direct results of inflammation, and, in his earlier writings,* ascribed their formation either to erosion of the walls of the ventricle, or to the bursting of abscesses into its cavity; but subsequently the occurrence of the case of Reynaud induced him to modify his opinions, and to admit the existence of the true aneurismal sac. Dr Thurnam has shown that most writers on these affections have advocated too exclusively one mode of origin in all cases of cardiac aneurism, and contends that the sacs formed in connection with the cavities of the heart are not only sometimes of the true and sometimes of the false form, but that they occasionally present all the varieties which occur in arterial aneurisms.

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* Art. Anéurysm du Cœur, Dict. de Méd. et de Chir. pratiques. T. ii. p. 398. 1829.

More recently, Rokitansky,* whose extensive field of observation entitles his opinions to attention, has treated of the aneurisms of the heart as of two kinds, both of which he regards as resulting from inflammation. In one of these the process is acute, and consists in inflammation of the endocardium, and of an adjacent layer of the muscular substance of the ventricle; which, being eroded or deprived of its power of resistance, gives way, while the corresponding external portion is gradually dilated into a sac, thus giving rise to the form of the disease described by Breschet as false consecutive aneurism.

The second variety he supposes to depend upon chronic inflammation, affecting not merely the endocardium and a thin layer of the muscular substance, but penetrating to a considerable depth, or even involving the whole thickness of a portion of the ventricular walls with the corresponding pericardium. In this form the muscular tissue disappears, and is replaced by pale fibres; and the whole portion of the ventricular parietes is rendered less resistant, and undergoes a gradual dilatation, forming a true aneurismal sac.

It is only by the examination of incipient aneurismal sacs, or of those of small size, that we can form a correct judgment as to their original modes of development; and it is probable, that, had writers on this subject confined their observation to such cases, less difference of opinion would have prevailed. Opportunities for their examination are by no means unfrequently presented to those who attentively look for them. During the period that I conducted the examinations of morbid bodies at the Royal Infirmary of Edinburgh, I met with three cases in which there existed small sacculated dilatations of portions of the left ventricle. A fourth case has more recently fallen under my notice; and I have been favoured with the reports relative to, and with inspection of the preparation of, three other cases which have occurred to Dr Thurnam since the date of his memoir. This series, the details of which I suppress, from a desire to shorten the paper, furnishes conclusive proof of the frequency of the true form of aneurism, and affords considerable insight into its process of development and progressive changes.

1*st*, In the earliest form in which the affection can be recognised, we observe thickening and opacity of the endocardium, with slight dilatation of the corresponding portion of the walls of the ventricle, and attenuation of the muscular substance, without any marked alteration of its texture.

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* Handbuch der Pathologischen Anatomie, 1844. Vienna. Das Herzaneurisma, 1 Band. p. 449.

2d, In a more advanced stage there is thickening and opacity of the endocardium, and conversion of a more or less thick stratum of the muscular substance into a dense tissue of a pale yellow colour, composed of fibres resembling those of tendon intermixed with the muscular tissue. The parietes have become more atrophied, and the corresponding portion of the cavity of the ventricle presents a more marked dilatation.

3d, At a still later period, together with the thickening and opacity of the endocardium, this membrane becomes intimately blended with the subjacent tissue, so as to be no longer traceable from it. The muscular substance throughout the whole or the greater part of the thickness of the ventricular parietes is converted into a dense, pale coloured fibrous tissue. The attenuation and atrophy of the walls of the ventricle are greater, and the dilatation of the corresponding portion of the cavity occasions a more or less marked prominence externally.

While these stages are in progress in the parietes of the ventricle, others are proceeding in its interior. The dilated portion of the cavity becomes, especially if it be somewhat circumscribed and bounded by a tolerably defined margin, the seat of coagula at first thin, loose, and dark-coloured; subsequently these become more firm, and of a paler colour, and at length the sac is found filled by distinctly laminated plates of decolorized fibrine. As the partial dilatation of the ventricular cavity increases and forms a more or less decided prominence externally, the visceral pericardium becomes implicated in the disease. At first it is only slightly opaque, and presents a rough surface from small granular exudations of lymph; these become thicker and more continuous, so as to constitute a distinct membrane; and at length adhesions are formed between the opposed surfaces of the visceral and reflected pericardium, either confined to the seat of the aneurism, or more rarely covering a large portion of the surface of the heart.

With the gradual progress of expansion, there can be little doubt that, as in arterial aneurisms, the internal portions of the cardiac aneurismal sacs are gradually removed, and we hence find that the larger tumours are almost invariably of the false-consecutive form. From my own observations and those recorded by Reynaud, Craigie, and Thurnam, I have, however, little hesitation in siding in the opinion of the last writer, that the partial aneurisms of the heart are most generally in their early stage of the true form, and originate in changes in the walls of the left ventricle, commencing with the altered condition of the endocardium described by Reynaud, and presenting, in a more advanced stage, the fibrous transformation of the muscular substance refered to by Cruveilhier. The nature of the diseased action by which these changes are effected is less clear. I am, however, inclined to believe that Rokitansky has correctly assigned their production to chronic inflammation of the endocardium and muscular walls of the ventricle. That endocarditis or pericarditis most frequently precedes the formation of cardiac aneurisms is rendered probable from the circumstance, that, in cases of incipient true aneurism, adhesions, thickening or opacity of the pericardium, and similar changes affecting the valvular folds, which are most commonly regarded as the results of inflammation, are of almost constant occurrence; while, in the advanced states of the disease, we have often evidences of a more decided character of previous inflammatory action. Reasons have before been assigned for associating the production of the aneurism in the present case with an attack of acute pericarditis. Dr Thurnam has related a case in which the mode of formation appears to have been similar, and expresses his belief that the cardiac aneurisms are generally connected with previous attacks of endocarditis or pericarditis; and Bouillaud and Rokitansky, as before stated, regard them as direct consequences of inflammation.

While, however, it is contended that the changes which precede the development of the true cardiac aneurisms are generally inflammatory, it is not asserted that their production may not occasionally result from other causes; and it seems probable that the imperfect nutrition of the walls of the ventricle, dependant on disease of the coronary arteries, may, as supposed by Mr Hodgson,* occasionally give rise to partial dilatation of its cavity.

The partial aneurisms occur only in connection with the left cavities of the heart, and especially of the left ventricle; and the apex and base are the portions of the latter in which they most frequently form. Breschet, who, from the limited number of cases which had been published before the date of his memoir, was led erroneously to conclude that the disease was almost exclusively confined to the apex of the ventricle, supposed this to be owing to the thinness of the ventricular walls in that situation predisposing them to partial ruptures under undue exertion.

Cruveilhier explained the common occurrence of cardiac aneurism at the apex of the left ventricle by stating, it is believed correctly, that the fibrous transformation of the muscular walls is here most frequently found. I may add that the change seems also by no means uncommon at the base, while it is extremely rare in the middle of the external wall and in the interventricular septum. It has before been stated, that dissenting from the opinion of Cruveilhier, who ascribes this transformation to a general law, by which, when there exists undue distension of any part of the parietes of the ventricle, the contractile is replaced by inelastic tissue, I am more inclined to agree with Rokitansky in regarding it as of inflammatory origin. Should this view be well founded it affords, it is conceived, a very ready explanation of the comparative frequency of the fibrous transformation in different portions of the walls of the ventricle. Since it is evident that, in proportion to the thinness of the interposed layer of muscular substance will be its tendency to be involved in diseased changes affecting the serous envelopes.

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* Provincial Medical and Surgical Journal, vol. vii. p. 200.

That aneurismal sacs are only formed in connection with the left cavities of the heart, may be ascribed to the greater tendency to inflammation in the endocardium of the cavities containing red blood, than in that lining the right cavities, combined with the increased force of the ventricular contractions, and the frequency of undue resistance to be overcome. Dr Thurnam has also referred to the circumstance of the right auriculo-ventricular valves allowing of regurgitation as more fully pointed out by Mr King, as probably in part explaining the non-occurrence of the cardiac aneurisms in the right ventricle.

IV. The false, though certainly of less frequent occurrence than the true form of aneurism, would appear occasionally to occur, and is most probably ordinarily the result of inflammatory softening or erosion of the endocardium and internal stratum of the muscular substance of the ventricle. It is nevertheless probable, that, agreeably to the supposition of Breschet, the internal membrane and a portion of the walls of the ventricle may, in some instances, undergo partial rupture from undue distension or exertion of the heart, without being preceded by material disease. Ruptures probably so occasioned have been recorded; and an example of the kind fell under my notice in a young man who died a few hours after the receipt of a severe injury of the chest, from the passage of a broad wheeled waggon over it. The ribs had on both sides been partially fractured, so as to depress the whole of the front of the chest, and the endocardium and muscular walls of the right auricle had undergone several partial ruptures, though the external membrane was uninjured.

The communication of the cavities of encysted abscesses with the interior of the left ventricle has been regarded by writers on aneurism of the heart, as affording a probable mode of their occasional production. So far, however, as I am aware, no very clear instance of an aneurism having been thus produced, has yet been recorded. In the following case, an abscess had formed in the walls of the auricles, and opened both into the base of the septum of the left ventricle, and into the sinuses of Valsalva, at the origin of the aorta, so that the blood had found its way into the cavity of the abscess, and was mixed with the pus. In this instance, the valvular apparatus at the origin of the aorta was too extensively implicated in the disease to allow the continuance of life. It affords, however, perhaps the nearest approach, which has been observed, to the formation of an aneurism by the communication of an abscess with the cavity of the ventricle.

CASE 2*d*.—Urgent dyspnœa; symptoms of delirium tremens; abscess in the walls of the auricles opening into the base of the left ventricle, and into the right and posterior sinuses of Valsalva at the origin of the aorta; pericarditis.

Lellis Hay, aged 35, was admitted into the Royal Infirmary of Edinburgh, April 22d 1848. Her habits were stated to have been very irregular; and having become pregnant, she had taken large doses of some mercurial preparation in the hopes of procuring abortion. Latterly she had abandoned herself to intemperance.

She had laboured under rheumatic pains for six or seven weeks before her admission, and when received, the right knee was found swollen and tender. While under treatment, she was in a state of great agitation and alarm, yet was perfectly intelligent when spoken to. She suffered from occasional paroxysms of difficulty of breathing, so urgent as to require that she should be supported in bed; but at times her respiration did not appear much affected, and she was able to lie down on her back or either side, indifferently. She died at 7 A. M. on the 4th of May. No report was taken of her case while in the hospital; the meagre facts just related having been obtained from the nurse of the ward, and her friends.

The body was examined in the presence of Dr Craigie, under whose care the case had been, May 6th, at 1 P. M., 54 hours after death.

The brain was free from any undue injection or congestion of the membranes or its substance;—the latter was of its usual firmness. Scarcely one drachm of fluid was found in the united ventricles, but the subarachnoid cellular tissue was copiously infiltrated with serum. The right lung adhered firmly to the parietes posteriorly by old cellular attachments. The left lung was firmly adherent by its base to the diaphragm, and the pleural sac contained eight ounces of clear fluid. Both lungs were healthy.

The abdominal organs were not allowed by the friends to be examined.

The pericardium was distended by opaque yellow-coloured serum, and the serous surfaces were covered by a layer of lymph, which, though very thin on the ventricles, formed a thick covering to the auricles, the origins of the large vessels, and in the course of the coronary arteries. The lymph was throughout soft and evidently very recently exuded. The heart was of medium size, and its walls and cavities natural in their dimensions.

The right auriculo-ventricular and pulmonic valves, as also the mitral valve, were healthy. The aortic valves were very greatly diseased. At the base of the right valve, there existed a small aperture, by which a probe could be passed into what appeared to be a dilatation of the corresponding sinus of Valsalva, and a large aperture existed in the fold of the valve on the left side. The posterior valve was more extensively diseased, its whole ventricular surface, especially at the base, being covered by soft masses of lymph of a reddish yellow colour; its fold was pierced by several large apertures, and its whole tissue was so soft as to give way on the slightest touch. Behind, or on its aortic side, there existed a cavity similar to that behind the right valve. The left aortic valve was healthy, except when it came in contact with the posterior, at which part it displayed a deposit of lymph, and was so soft as to give way under the finger. The cavities situated behind the right and posterior valves, which resembled dilatations of the sinuses of Valsalva, proved on examination to have been produced by the separation of the bases of the valves from their attachment to the fibrous zone. This was evident in reference to the right valve from the persistence of a distinct margin, marking the former point of attachment of its fold. The sacs thus produced at the origin of the aorta communicated with a cavity situated in the walls of the auricles. This cavity was bilocular, being partially separated into two portions by the septum of the auricles. It formed distinct swellings projecting into each auricle. The portions into which it was partially divided were each of sufficient size to have lodged a cob nut, and appeared to be lined by a distinct membrane. They contained pus mixed with coagulated blood. The portions of the walls of the auricles, bounding the sacs towards the auricular cavities, were much thickened, being infiltrated with serum; while on the external or pericardial side, the matter pointed, and could be seen through the attenuated parietes of the abscess.*

The exact nature of the disease in this case was not at first very apparent. It seemed, however, most probable that the muscular substance of the auricular walls had become inflamed, in connection with the rheumatic inflammation affecting the joints; that matter had formed in this situation, which, burrowing towards the attachment of the auricles and ventricles, had perforated the base of the *septum ventriculorum*, opening below the right semilunar valve into the cavity of the left ventricle; and had then penetrated behind the right and posterior valves into the corresponding sinuses of Valsalva. The inflammation of the serous covering of the heart seemed to have resulted from the extension of the disease on the outer side of the abscess.

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* The preparation of this case is preserved in the Museum of the University of Edinburgh.

In this instance the destruction of the posterior semilunar valve proved fatal to life, almost simultaneously with the opening of the matter into the left ventricle, and origin of the aorta. Had, however, the valve been less implicated in the diseased process, there seems reason to believe that life might have been for some time prolonged. Under these circumstances, a portion of the blood would, with the ventricular systole, have found its way from the left ventricle into the sac of the abscess in the walls of the auricles, and behind the semilunar valves into the sinuses of Valsalva, and regurgitation would have taken place from the aorta by the same course with the ventricular diastole, thus presenting a form of disease very similar to the cases described by Dr Hope, which Dr Thurnam regards as analogous to the so-called dissecting aneurisms of arteries. The imperfect observations of the symptoms present during life in the case is to be regretted, since, independently of the interest attaching to it in reference to the probable formation of cardiac aneurism, instances of suppuration, whether diffused or circumscribed, in the walls of the heart, are of extremely rare occurrence. If, indeed, we except the very imperfect notices of cases of this kind, recorded in the works of the older anatomists, and the instances like those described by Corvisart, in which the affection of the muscular substance was evidently secondary to inflammation of the pericardium, our knowledge of diffuse suppuration and abscess of the walls of the heart is confined to about fourteen recorded cases, of which ten are, like that here related, instances of circumscribed abscess.*

V.—*Aneurism of the Left Auricle.*

Partial aneurism, though most frequently observed in the left ventricle, is not confined to that cavity, a few instances having occurred of a similar affection of the left auricle.

Dionis† in 1716, recorded a case of very great dilatation of the left auricle, in a soldier under the care of M. Manne in the hospital at Avignon. Burns[*] and Abernethy[†] have related instances of contraction of the left auriculo-ventricular aperture, in which the appendix of the auricle was found dilated, and lined by fibrinous clots; and more recently cases of this description have been noticed by Hodgson,[‡] Sir A. Cooper,[§] Cruveilhier,[||] Hope,[¶] &c., and others have been referred to by Dr Thurnam.

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* Laennec, Traité, ii. p. 303. Trans. 4th edit. p. 577.
Neubert, Hufeland, Journ. Band 57, p. 97. Nov. 1823.
Andral, Précis d'Anatomie Path. T. ii. p. 324. Trans. vol. ii. p. 343.
Latham, Medical Gazette, vol. iii. p. 118.
Casimir Broussais, quoted from the Annales de la Med. Phys., by Lettré, in Dict. de Med. art. Cœur, and by Bouillaud, Traite, T. ii. p. 262.
Krauss, quoted by Lettré.
Graves, Clinical Medicine, 1843, p. 641.
Gintrac, Bullet. de l'Acad. de Med. T. viii. p. 856. 1842 and 1843.
Chambers, Lancet. July 1844. p. 557.
Chance, Lancet. May 16, 1846. In this paper a preparation is referred to as existing in the museum at Fort Pitt.
Cases of general suppuration have been recorded and referred to by Laennec.
Raikem, see Lettré and Bouillaud, supra.
Stanley. Med. Chir. Trans. vol. vii. p. 323.
Simonet, Lettré and Bouillaud.
Salter, Med. Chir. Trans. vol. xxii. p. 72.

† Anatomie de l'Homme. Paris, 1716, p. 713.

The following case affords an example of this kind.

Case 3*d*.—Symptoms of cardiac disease of three years' duration; old paralysis of the left side; chronic softening of the right *corpus striatum* and *thalamus nervi optici*; great contraction and permanent patency of the left auriculo-ventricular aperture; dilatation of the appendix of the left auricle, and obliteration of its cavity by fibrinous coagula.

The subject of this case was a man 36 years of age, who had been several times under treatment in the Royal Infirmary of Edinburgh, and died in October 1842, under the care of Dr Paterson. He had laboured, for upwards of three years, under symptoms of cardiac disease, characterised by pain in the region of the heart; difficulty of breathing, greatly aggravated by any exertion and especially by ascending any height; cough with copious frothy, and occasionally bloody expectoration; and he had several times had copious hæmoptysis. These symptoms became more aggravated towards the last periods of his life; the dyspnœa often became so urgent as to prevent his lying down,—the pain in the region of the heart was acute and lancinating; and fluid was effused into the cavities of the abdomen and thorax, and into the external cellular tissue. He had been paralytic on the left side of the body for eighteen months before death; and was subject to fits of giddiness and partial loss of sight, but never had an apoplectic seizure. The cardiac impulse was perceptible over an unusual extent of the præcordial region; the dulness on percussion exceeded considerably its normal limits; and a loud rasping murmur was heard with the systole of the heart, and was most intense two inches below and to the left of the nipple. The pulse was latterly quick, feeble, and irregular.

On examination after death there was found considerable subarachnoid effusion, but the ventricles contained only one drachm of fluid. On the external surface of the right hemisphere there existed some old lymph, and from this point a softened portion, surrounded by indurated cerebral substance, was traced to the external part of the right *corpus striatum* and *thalamus opticus.* There was also a more recent softening on the right lobe of the cerebellum. The arteries of the brain were diseased. Fluid existed in the pleural sacs, pericardium, and abdominal cavities; and the lungs, liver, spleen, and kidneys were greatly engorged with blood.

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[*] Diseases of the Heart, &c. 1809, p. 135 and 168.
[†] Medico-Chir. Trans. vol. i. 1812, p. 32.
[‡] Diseases of Arteries and Veins. 1815, p. 83.
[§] Lectures by Tyrrel, p. 28. 1825.
[||] Anatomie Pathologique, Liv. xxii.
[¶] Diseases of the Heart. 1832, p. 509, case 25.

The heart weighed 17½ ounces. Its right cavities were very greatly dilated, and the walls of the right ventricle much increased in thickness. The right valves were healthy. The left ventricle was considerably increased in capacity and its walls hypertrophied; the aortic valves were rough and somewhat cartilaginous, but were capable of performing their functions. The left auriculo-ventricular aperture was reduced to a mere slit, half an inch in length; and the edges of the opening, with the folds of the valve, were thickened, rigid, and partially cartilaginous; the *cordæ tendineæ* were shortened and thickened; and the *columnæ carneæ* were considerably hypertrophied. The left auricle was very greatly dilated and its walls hypertrophied, the muscular substance being much more distinct than usual. The endocardium was throughout opaque, and much thickened. The appendix was much dilated, and formed an oblong tumour 2½ inches long, and 1 inch and 10 lines deep, which was separated by a distinct furrow from the rest of the auricle, and projected in front of the base of the heart. Its walls consisted of a dense whitish coloured fibrous tissue, having little or nothing of their natural muscular appearance. Its external and internal serous membranes were much thickened. The cavity was entirely filled by firm and partially laminated and decolorized coagula, so that the whole felt perfectly solid. The aperture by which the appendix communicated with the sinus of the auricle was much decreased in size, being not more than half an inch in diameter, and was completely closed by a portion of firm fibrinous coagulum.

In most of the published cases of aneurismal dilatation of the left auricle, as in that just described, the auriculo-ventricular aperture appears to have been more or less contracted. The appendix is not exclusively the seat of the disease, the sinus having in several instances been affected. Generally the walls of the dilated portion of the auricle have been lined by fibrinous clots, and in most instances no distinct line of separation has been found between the dilatation and cavity of the auricle. An example of a distinctly sacculated aneurism has, however, been referred to by M. Cruveilhier, as shown at the Anatomical Society of Paris by M. Chassaigniac; and more recently a short notice of a similar case has been published by Mr Fenwick in the Lancet for the 24th of January of this year. In the latter cases also the left auriculo-ventricular apertures and valves were free from disease. The preparation of an aneurism of the appendix of the left auricle contained in the College of Surgeons' Museum, and which has been referred to by Dr T. Davis, and more fully described by Dr Thurnam while in the possession of Mr Langstaff, is in every respect very similar to the case I have described.

VI. The means of diagnosis of the partial aneurisms of the heart constitutes the great desideratum which remains to be supplied in reference to these affections.

The general symptoms induced by this form of cardiac lesion would appear to be in many cases very unimportant, and when more distinctly marked, do not differ in any very obvious particular from those dependent on other diseases of the heart. It is, indeed, only by the physical signs that we can expect to ascertain during life the existence of these affections; and these, though easily assigned on theoretical grounds, have been in very few cases carefully and accurately observed. M. Gendrin informs us that in four cases he observed a murmur with the impulse of the heart, ceasing abruptly with the systole, and followed by an interval of silence, after which a short diastolic murmur succeeded. When, however, we bear in mind the variety of other lesions which might give rise to similar signs, and the frequency with which the cardiac aneurisms are combined with valvular or other organic diseases of the heart, the value of these signs as the means of effecting a differential diagnosis cannot, in the present state of our knowledge, be regarded as great. The remark, indeed, with which M. Breschet concludes the discussion of this subject in his memoir seems not less applicable to the present time than to that in which it was written. "Le diagnostic de pareilles tumours anéurismales est donc jusqu' ici, fort difficile a établir. La connaissance de l'existence de ce genre de lesion organique, les circonstances anamnestiques, peuvent seulement mettre sur la voie, et rendre l'observateur plus attentif."

ART. II.—*On the Age of Puberty in the Island of Madeira.*

By JOHN ROBERTON, Esq. Manchester.

MADEIRA, situated in about latitude 32°, is said to consist of a huge mass of basalt, which, rising abruptly from an unfathomable ocean, terminates in a cluster of mountains, the highest of which, the Pico Ruivo, attains an altitude of nearly 6000 feet.

This island is distant from the west coast of Africa four hundred miles, is of a moderate area, being about the size of Huntingdonshire or 360 square miles, and contains a population estimated at 80,000, chiefly of Portuguese origin, and in part negroes in a state of slavery. To say that the island is a cluster of mountain peaks implies that it has numerous valleys: and these are profusely fertile in almost every kind of produce, tropical and European, rendering it, when we consider the favourable climate, one of the most delightful abodes of man on the globe

Though famous for its rich flavoured wine, it is still more famous for its climate, which, for mildness and equability, is without a rival. Although several degrees farther south than any part of Europe, the mean annual temperature of Funchal, the chief town, is 64°; being only about 5° warmer than the average of the Italian and Provençal climates. This moderate mean temperature is owing principally to the coolness of the summer, which is only 7° warmer than that of London, whilst its winter is 20° warmer; and whilst the Madeira winter is 12° warmer than in Italy and Provence, the summer is nearly 5° cooler.* A correspondent informs me that the heat in the island during the present summer (1846), has been under that of England, the thermometer at Funchal not having risen above 85°. Nevertheless I would call attention to the mean range of the thermometer, from which it appears that Madeira really possesses a higher temperature, taking the year throughout, than any part of Europe. Thus the mean of London is 50°, Paris 51°, Montpelier 57°, Florence 59°, Rome 60°, Naples 61°, Cadiz 62°, but that of Madeira 64°.

Bearing in mind the latitude and the temperature of the island, as just stated, we are now prepared to consider the following facts with reference to the age of female puberty in the white native population.

Chancing a couple of years ago, to have a friend about to visit Madeira for the benefit of his health, I begged him to do me the favour of getting some competent person to make inquiries on the subject of puberty in that island. This he engaged to do, and having become acquainted with a physician of zeal and energy in practice there, Dr F. D. Dyster, he informed me on his return that I should probably in a few months receive a communication from Madeira. Accordingly, in the beginning of the present year, I had a letter from Dr Dyster, furnishing me with a table of eighty-four instances with reference to the period of puberty, collected from amongst the lower classes. The people have no accurate notion of their ages, he informed me, nor of the date of distant events, and consequently he was obliged to restrict his inquiries to such females only as could give a credible account of themselves. Again, in the present month (August), I have been most kindly favoured with a second communication from the same gentleman, containing a further table, which, when embodied with the former, furnishes instances amounting in all to 228. This body of facts, it will be seen, agrees, generally speaking, with the table for England; except, indeed, that the mean age of puberty in Madeira is six or seven months later than in the former. However, I do not think that there is actually such difference as this, for were the data as ample as we possess for England, the discrepance, there can be little doubt, would be found to disappear.

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* Clark on the Influence of Climate, 2d. edit. p. 185.

Ages.	Period of Puberty. Madeira.	Period of Puberty. England.
9	—	3
10	—	14
11	2	19
12	11	35
13	19	66
14	35	99
15	67	104
16	40	85
17	21	54
18	12	34
19	17	16
20	4	8
21	—	2
22	—	1
	228	540
	Mean, 15 years 5 months nearly.	14 years 10 months.

It will here be seen that the instances, excepting at the earlier ages, are distributed much the same in the column of figures for Madeira, as they are in that for England. At the earlier ages, indeed, of 11 and 12 years, the proportions who arrive at puberty in Madeira would seem to be considerably less than in England. Moreover, in the table there are no instances at all for Madeira opposite the ages of 9 and 10 years; but, as was before remarked, more ample data would perhaps remove the difference. In corroboration of this supposition I would point out a coincidence between the two columns of figures, namely, that the maximum number of instances, in each of the columns alike, is opposite the age of 15. Be this as it may, however, the notion—the old universally received opinion—that puberty is earlier in Madeira and the whole of the south of Europe than in our own country, receives no countenance from these data—rather, seemingly, the contrary.

Concerning the age for the cessation of the catamenia in Madeira, Dr Dyster states that he has only been able to obtain fourteen instances on which he could depend, and that these give the average age as 47 years and 7 months.

In his second letter Dr Dyster supplies a fact which will be considered of some consequence, as it directly militates against a notion that has ever prevailed, viz. that menstruation is always more profuse in southern latitudes. These are his words:—"There is a point which I think is somewhat singular, considering the simple habits of the people and the glorious climate, namely, the exceedingly frequent derangement of the uterine function. Menstruation is rarely profuse—very commonly deficient both in quantity and duration. Anæmia is, in general, at the root of the evil; and aloes and iron the most efficacious remedies." As some explanation, it ought to be stated that the lower orders of Madeira are indifferently fed; and Dr Dyster considers that their physical development is imperfect and tardy. Anæmia would seem to be a common malady.

The paper which I wrote concerning puberty in Greece (published in a former number of this Journal) was by some thought to be deficient in facts—I fear not without justice. Now the present communication may perhaps be accepted as supplemental to *that* in reference to Greece. The climate and the people do not differ much in Madeira and in Greece, and the valuable body of facts supplied by Dr Dyster, while it proves what is the average age of puberty as regards the island of Madeira, may no less serve to elucidate the same point as respects Greece and the south of Europe in general.

In my paper, just referred to, I remarked that it was worthy of notice, that among the ancient Greeks the women do not appear to have been considered as in any sense precocious. On the contrary, that the age of marriage was probably as late in ancient Greece, as it is in the most refined nations of Europe at the present day. I am now enabled, in favour of this opinion, to quote the testimony of the Greek physician Paulus Ægineta, supposed to have written in the sixth century, whose works are in course of publication, in a valuable translation, by the Sydenham Society. "With most women," says Ægineta, "the menstrual discharge begins about the fourteenth year of their age; a few have it earlier, in their thirteenth or twelfth; and not a few are later than their fourteenth in having it. There is no limited time for the continuance of it; many having it only for two or three days; most women for five days, some for seven, and a very few have it for twelve days. The *menses* cease about the fiftieth year of age, a few have them till sixty, and with some they begin to disappear about thirty-five, particularly with such as are fat." Book iii. Sect. lx.

This is surely a remarkable passage, for the precision, brevity, and yet fulness with which the author's views are enunciated. He manifestly understood the subject he is treating of better than his successors have done; for instead of representing puberty as occurring at or about some one year of age, as has generally been the case with the moderns, he seems to have been aware that this occurs through a considerable range of ages. He appears also to have known that the cessation of the function varies in like manner even more as to age than does its commencement. And, further, in placing the mean age of the cessation at *fifty*, instead of, as more recent writers, at forty-five, he proves that he must have written from observation. Nor have we a word in this ancient author concerning the precocity of the nations bordering on, or inhabiting the islands of, the Mediterranean—nothing to corroborate Mr Frederick Strong's assertion, formerly quoted and commented upon, "that nature is so extremely precocious in Greece, that females attain the age of puberty at ten or eleven years."*

ART. III.—*Case of Phosphorescence in the Human Body.* By HENRY M'CORMACK, M. D.

INSTANCES of human phosphorescence are so rare, that it is incumbent on any one who meets with the same to bring them forward.

The subject of the following short notice was a male infant of about sixteen months old. The parents are highly respectable inhabitants of Portaferry; the child was under the charge of Drs Chirmside, father and son, of that town. I had seen it some two or three months previously in the presence of Dr Thomas Chirmside. This was casually, however, as I had proceeded to Portaferry, not to visit the child, but on account of other professional matters, in consultation with my medical friends already named.

My attendance at Portaferry had ceased, when I received a communication from the father of the child, which I had better give in his own words. After stating that the infant suffered from teething, my correspondent proceeds to say, " he had been under the care of both doctors without obtaining relief. Dr Thomas Chirmside thinks there is derangement of the bowels also. There is no inflammation in either pulse or skin; both are soft. He has a cough which might proceed from cold, and his breathing was very much hurried. For this an emetic was administered, and an irritating liniment rubbed on his breast. The breathing is relieved, but the cough continues. His nurse observed last night some phosphorescent light about his hips in raising him in bed; the sheets and her own hands were covered with it on drawing them from under him. She called Sarah (the child's mother), who saw a little of it on his hips, both before and after the candle had been lighted." The child's parents, as might be supposed, were uneasy. They knew well enough that it was a rare occurrence, and wished to be informed if it were symptomatic of any disease. Both parents came up here a couple of days since, and

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* *Corrigendum.*—In Mr Roberton's paper in our last Number the following passage near the foot of page 59,—" From the foregoing table and from tables published in several former papers, I have drawn up the following summary of the results,"—must be read not in its present connection, but as immediately *preceding* the table of " Puberty in Hindostan and in England," at top of p. 60.

I had an opportunity of asking for further particulars. There was no moisture in the bed, nor any thing that could account for what had occurred. The nurse, on first perceiving the phenomenon, was greatly alarmed. She slept in a room over the parents, and summoned them by knocking on the floor. Nurse told the child's mother, that when she first looked the legs were luminous, but they were no longer so when the mother came up. The appearance, so far as I can learn, must have worn very much that which is produced when a surface is rubbed with phosphorized oil, but I have already stated there was no moisture; and I may add, there was no smell. The nurse, an ignorant country woman, knew nothing of phosphorized oil; and I need hardly observe, that it was never ordered. The phenomenon may have occurred oftener than once, but it was only once observed. On asking the lady, whose child it was, whether there had been any recurrence, she said that a spark had once flown to her hand from the infant's body. This occurrence is purely electrical, and though curious, is of vastly greater frequency than the other.

A few examples of human phosphorescence are to be met with in ancient authors; but I know not at present where to seek for them. Sir H. Marsh, along with some interesting observations, has published one instance, which, he says, is unique. [Of this an abstract is given in the fifty-eighth volume of this Journal, p. 497.] Human phosphorescence, after death, has been repeatedly witnessed,—*e.g.* in the dead bodies at La Pitie in Paris, in human bodies, and often enough in grave yards if from the same source. Every one is aware of the frequency of phosphorescence in the fishy tribes. To explain the expulsion of phosphorus from the skin—the how and the why—even in these recondite days, when the arcana of animal chemistry have been so widely laid open, would be difficult, perhaps impossible. I am satisfied with the more humble duty of recording the fact.

Belfast, June 1846.

ART. IV.—*The Power of the Mind over the Body: An Experimental Inquiry into the nature and cause of the Phenomena attributed by Baron Reichenbach and others to a "New Imponderable."* By JAMES BRAID, M.R.C.S.E., C.M.W.S., &c.

FEW publications have lately issued from the press so well calculated to excite general interest and inquiry, as Baron Von Reichenbach's "Researches on Magnetism." The high reputation of the author, as well as that of his learned translator and annotator, Professor Gregory, who has furnished a condensed view of the subject in an English dress, were all calculated to produce an effect—the greater, because the subject discussed was represented as bringing under our notice "a new imponderable," through which we should realize a clear and satisfactory solution of many problems in the mental and physical constitution of man, which had puzzled and perplexed alike the savage and the sage from the earliest ages.

The vast interest which the above-named tract has created, is evinced by the extent to which it has been quoted, referred to, and reviewed in our numerous periodicals. Nor is it devoid of interest to observe the various awards of these different authorities. Thus, one quotes at great length, and not only with seriousness sets down the whole as fully established, but, moreover, with much self-complacency exults in the proof thereby adduced of the correctness of all the most extreme views he and other Mesmerists may have promulgated on certain points of this keenly debated science; whilst another writes a clever burlesque article, holding up the whole speculation as worthy only of unsparing ridicule. Between these two extremes, again, we have every grade of approval or scepticism.

In the midst of this discordant state of speculative opinions, it appeared to me that you had adopted the most prudent course, by publishing, in your April number, ample extracts from Dr Gregory's "Abstract," so that the statements and opinions therein advanced might be made known to your readers, whilst you abstained from offering any opinion of your own, as to whether they were facts or not, until the experiments had been repeated and confirmed by others. The results which I have now to submit will tend to prove the propriety of the cautious course adopted by you.

On the first announcement of Dr Gregory's abstract of Baron Reichenbach's "Researches on Magnetism," I lost no time in procuring a copy, which I perused with intense interest. I had not proceeded far, however, when my experience with hypnotic patients enabled me to perceive a source of fallacy, of which the Baron must either have been ignorant, or which he had entirely overlooked. From whatever cause this oversight had arisen, I felt confident that, however carefully and perseveringly he had prosecuted his experiments, and however well-calculated they had been for determining mere physical facts, still no reliance could be placed upon the accuracy of conclusions drawn from premises assumed as true, where especial care had not been taken to guard against the source of fallacy to which I refer, viz. the important influence of the mental part of the process, which is in active operation with patients during such experiments*. I therefore resolved to repeat his experiments, paying the strictest attention to this point; and, as I had anticipated, the results were quite opposed to the conclusions of Baron Reichenbach. It is with considerable diffidence that I venture to publish an opinion opposed to such high authority; but I shall briefly state the grounds of my own belief, and leave it to others to repeat the experiments, and determine which opinion is nearer the truth. The observations which I have to submit may, moreover, prove suggestive to others, and enable them not only to avoid sources of fallacy with which I am familiar, but may also lead to the detection of many which may have escaped my own observation.

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* Let it not be supposed that I intend by these observations to impute to Baron Reichenbach any want of ordinary caution, or even the strictest desire to guard against sources of fallacy of every sort. From what is recorded at pp. 13, 50, and 81, of the "Abstract," it is obvious that he had taken great precautions, and such,

The great aim of Baron Reichenbach's researches in this department of science has been to establish the existence of a new imponderable, and to determine its qualities and powers in reference to matter and other forces, vital and inanimate. It unfortunately happens, however, that the only test of this alleged new force (with one solitary exception, and that as I thought by no means a satisfactory one), is the human nerve; and not only so,

indeed, as would have been quite adequate to effect such purpose under ordinary circumstances. Moreover, I most cordially admit that a better devised series of experiments, or a more laborious and painstaking effort, for determining the question on strictly inductive principles, I have never met with in any department of science. Nor is small praise due to Professor Gregory for the masterly manner in which he has performed his part of the task; still, when the extraordinary acuteness of the organs of special sense are taken into account, to which reference is made at p. 2 of the " Abstract," where it is admitted that " smell and taste become astonishingly delicate and acute ;" vision so irritable that patients " are able, in very dark rooms, to distinguish not only the outlines, but also the colours of objects where healthy people cannot distinguish anything at all ;" and that " patients hear and understand what is spoken three or four rooms off;" I say, when these facts are taken into consideration, coupled with the excitable state of their minds, it will readily be admitted, that with all the precautions resorted to by Baron Reichenbach, various ideas may have been conveyed to his secluded patients, through the ordinary channels, of which he had no suspicion. For example, slight sounds in conducting the experiments, may have been overheard by the patients, and indicated to them changes in the arrangements, of which the operator had no notion ; tones of voice, in proposing questions, may have had an equally certain and unintended influence ; and it is an undoubted fact that, with such subjects, the slightest change in the concomitants always suggests a change in the fundamental or previously existing idea. Such being the case, I can imagine no satisfactory mode of determining the subject with such patients as the Baron experimented with in the stair-case, but having them placed beyond the range of hearing, even to their quickened ears. The conducting wires should be securely fixed by thumb-screws near their distal ends, so as to prevent any vibration taking place beyond, when changing arrangements ; which should be performed with the least possible noise ; and a single sound of a bell should be the only signal for an answer, unless the patient should wish voluntarily to express some intelligence as to impressions, which he ought to be at liberty to do at any time. I would also suggest that there ought to be no regular order in the experiments ; and that changes should be made without sounding the bell ; and occasionally, that the bell should be sounded repeatedly without any change having been made. On no account should the experiments be conducted where the patients can have ocular observation of the features or gestures of the investigators, even in apartments apparently dark. Such are the rigorous conditions I should consider requisite in the investigation of this subject, so as to guard against all obvious sources of fallacy.

but it is further admitted that its existence can only be demonstrated by certain impressions imparted to, or experienced by, a comparatively small number of highly sensitive and nervous subjects.* But it is an undoubted fact that with many individuals, and especially of the highly nervous, and imaginative, and abstractive classes, a strong direction of consciousness to any part of the body, especially if attended with the expectation or belief of something being about to happen, is quite sufficient to change the physical action of the part, and to produce such impressions from this cause alone, as Baron Reichenbach attributes to his new force. Thus every variety of feeling may be excited from an internal or mental cause—such as heat or cold, pricking, creeping, tingling, spasmodic twitching of muscles, catalepsy, a feeling of attraction or repulsion, sights of every form or hue, odours, tastes, and sounds, in endless variety, and so on, according as accident or intention may have suggested. Moreover, the oftener such impressions have been excited, the more readily may they be reproduced, under similar circumstances, through the laws of association and habit. Such being the fact, it must consequently be obvious to every intelligent and unprejudiced person, that no implicit reliance can be placed on the human nerve, as a test of this new power in producing effects from external impressions or influences, when precisely the same phenomena may arise from an internal or mental influence, when no external agency whatever is in operation.

In order to guard against this source of fallacy, therefore, I considered it would be the best mode to throw patients into the nervous sleep, (the term adopted by me in preference to mesmeric sleep, for reasons to be explained presently), and then operate on such of them as I knew had no use of their eyes during the sleep (for some patients have), and to take accurate notice of the results when a magnet capable of lifting fourteen pounds was drawn over the hand and other parts of the body without contact, after the manner described as performed by Baron Reichenbach in his experiments.

I experimented accordingly, and the results were, that in no instance was there the slightest effect manifested, unless when the magnet was brought so near as to enable the patient to feel the abstraction of heat (producing a sensation of cold), when a feeling of discomfort was manifested, with a disposition to move the hand, or head, or face, as the case might be, from the offending cause. This indication was precisely the same when the armature was attached, as when the magnet was open; and in both cases, if I suffered the magnet to touch the patient, instantly the part was hurriedly withdrawn, as I have always seen manifested during the primary stage of hypnotism, when the patients were touched with any cold object. Now, inasmuch as patients in this condition, generally, if not always, manifest their perceptions of external impressions by the most natural movements, unless the natural law has been subverted by some preconceived notion or suggested idea to the contrary; and as I have operated with similar results upon a considerable number of patients, we have thus satisfactory proof that there was no real attractive power, of a magnetic or other nature, tending to draw the patient, or any of his members, so as to cause an adhesion between his body and the magnet, as between the latter and iron, as Baron Reichenbach had alleged. I conclude, therefore, that the phenomena of apparent attraction manifested in his cases were due entirely to a mental influence; and I shall presently prove that this is quite adequate to the production of such effects.

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* Such, at least, appeared to be the conviction as set forth in the *Abstract*. I soon ascertained, however, that, as respects common sensation in particular, many of the phenomena could be readily demonstrated even in healthy persons,—but on principles admitting of a totally different explanation from that of Baron Reichenbach, as shall be presently illustrated and proved.

By referring to your April Number, and comparing the results recorded by the different patients, no one can fail to be struck with the remarkable discrepancy in their description of what has been alleged as a physical fact. All expected, as I presume, to see light, and they saw light or flames accordingly; but let us remark another result: Mdlle. Nowotny, in her most sensitive state, saw luminous vapour surrounded by rays which were in a constant shooting motion. "The colour of the light was nearly pure white, sometimes mixed with iridescent colours." The appearance was the same at both poles; the length of the flames, about one-half or three-fourths of an inch. Mdlle. Maix, in her ordinary state, saw flames (from the same magnet I presume,) a hand-breadth in height, and when in the more sensitive state they were much larger; and she now saw currents of flame proceeding from the whole external surface of the magnet, but weaker than at the poles.

Mdlle. B. Reichel saw the magnet giving out light, not only when open, but also when closed. When open, the flames from the poles were about eight inches high, those at the joining of the plates of the magnet about a finger's breadth in length. "These small flames appeared blue, the chief light was white below, yellow higher up, then red, and green at top." As with the others, both poles seemed to have given out similar appearances of light and flame. This same patient being experimented on with the above straight bar magnet, about a foot and a half long, we are told that, "at the pole pointing to north, or negative end of the magnet, the flame was larger than at the opposite end;" and we are further told that "it was red below, blue in the middle, and green at the top." When we advance to the flames seen to be given out by crystals, from the human hand, and other forms of matter, we have equally discordant descriptions as to colour and size of the flames as seen by different sensitive subjects. Now, to my mind, these discordant statements as to the colour and size of the flames, are quite fatal to the notion of such representations proving a physical fact; and in an especial manner is that remark applicable to the statement of Mdlle. Reichel, who not only saw the colour different which was emanating from a straight bar magnet from that of the horse-shoe variety, but also described the size of the flame as larger from the north pole of the straight bar magnet than from the other end, whereas it was always seen by her, as well as by others, to be the same size at both poles of a horse-shoe magnet. If there be a physical reality in these alleged flames and colours, there ought to be no discrepancies of this sort; and the fact of such discordant statements having been made will tend to prepare the mind of the reader for the solution of the problem which I have now to submit.

In prosecuting the inquiry, Baron Reichenbach considered he had not only proved the existence of this new force, which produced all the physical effects enumerated, with streams of light from the poles, and the power of attracting the human body, and adhering to it, as steel to the magnet—I say he alleged that he had established such a force, not only as residing in the magnet, in addition to the ordinary magnetic force, but moreover, that it was found equally active in crystals, where it existed quite pure and distinct from ordinary magnetism. He also now ascertained that his subjects could discover "that from the finger points of healthy men, fiery bundles of light streamed forth, exactly as from the poles of magnets and of crystals visible to the sensitive." He alleges, moreover, that he had proved that where it was passive, it could be excited into activity by the sun's rays, by the moon's rays, by starlight, by heat, by chemical or mechanical action, and finally, that this luminous or phosphorescent appearance, and certain other peculiar properties, might be discovered by the highly sensitive in almost every place, and from nearly every object or form of matter, solid or fluid, animate or inanimate. Also, that it could be conducted through all other matter, and that all substances, not naturally actively charged with it, could be so temporarily, by proximity and contact of those which were actively charged.

I have already stated the wonderful power of the human mind, when consciousness is strongly directed to any part of a sensitive person, in changing physical action, and leading the subject to attribute to an external cause what may have arisen entirely from an internal or mental cause. It has also been stated that, when I resorted to a mode of operating which rendered the subjects more highly sensitive to external influences, and at the same time was calculated to obviate any source of fallacy, as to mental emotion or expectation being directed to the part from their seeing what was being done, the results were in direct opposition to what was represented as having been realized by the Baron. I have particularly adverted to this in respect to the alleged attraction of the magnet for the human frame; I have proved it to be equally so in respect to the human hand, and crystals, &c., where all sources of fallacy are guarded against. In my experience, moreover, with such cases, no light or flames have been perceived by patients either from the poles of a magnet, crystals, the points of the fingers, or other substance, unless the patients have been previously penetrated with some idea of the sort, or have been plied with such questions as were calculated to excite motions, when various answers were given accordingly, and when in the sleep, there appeared an equal aptitude to see something when neither magnet nor fingers were in the direction indicated, as when they were —a clear proof that the impressions were entirely imaginary, or mental in their origin.

I shall now proceed to detail the results of experiments with patients when wide awake, and when they had an opportunity of seeing what was being done, and expected something to happen; and also when the same patients saw nothing of what was doing, but supposed I was operating, and consequently expected something to occur.

With nearly all the patients I have tried, many of whom had never been hypnotised* or mesmerised, when drawing the magnet or other object slowly from the wrist to the points of the fingers, various effects were realized, such as a change of temperature, tingling, creeping, pricking, spasmodic twitching, catalepsy of the fingers, or arm, or both; and reversing the motion was generally followed by a change of symptoms, from the altered current of ideas thereby suggested. Moreover, if any idea of what might be expected existed in the mind previously, or was suggested orally, during the process, it was generally very speedily realized. The above patients being now requested to look aside, or a screen having been interposed, so as to prevent their seeing what was being done, and they were requested to describe their sensations during the repetition of the processes, similar phenomena were stated to be realized, even when there was nothing whatever done, beyond watching them, and noting their responses. They believed the processes were being repeated, and had their minds directed to the part, and thus the physical action was excited, so as actually to lead them to believe and describe their feelings as arising from external impressions.

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* *Hypnotised* is the term adopted by me to designate the state of *nervous sleep* induced by my usual processes. I commenced my experimental investigation of mesmerism as a decided sceptic; but when I had fully satisfied myself of the reality and peculiarity of the condition,—that they did not depend upon the transmission from the operator to the patient of any magnetic or special influence, as alleged by the mesmerists,—that the power was capable of being turned to important purposes in the relief and cure of disease; and when I had, moreover, ascertained that the phenomena producible by my processes did not include the *higher* phenomena, as they are called, of the mesmerists, such as clairvoyance, seeing through opaque bodies, mesmeric intuition, transposition of the senses, &c. &c., I deemed it prudent to adopt such conventional terms as might enable me to express my own views and convictions to the profession and the public, without being confounded with those who believed in, and contended for, all the extreme views of the mesmerists. I hoped thus to insure for the *ordinary and practically useful phenomena, a patient investigation*, which had been refused to those who contended for extreme views, respecting phenomena of rare occurrence and doubtful character. I therefore adopted the conventional term "Neuro-Hypnotism," or, for the sake of brevity, simply "Hypnotism or Nervous Sleep," to designate a peculiar condition of the nervous system, into which it can be thrown by artificial contrivance, different in several respects from common sleep, or the ordinary waking condition. Strictly speaking, it comprises not *one state*, but a *series* of *stages*, varying in every conceivable degree, from mere reverie to profound nervous coma, with entire abolition of consciousness, and voluntary power on the one hand, to an almost incredible exaltation of function of the organs of special sense, and of intellectual and voluntary power, on the other. The phenomena are partly mental and partly physical,—voluntary, involuntary, or mixed, according to the stage of the sleep. There is also as great difference in the susceptibility to the hypnotic

The above fact was most remarkably evinced in a young gentleman, twenty-one years of age. I first operated in this manner on his right hand, by drawing a powerful horse-shoe magnet over the hand, without contact, whilst the armature was attached. He immediately observed a sensation of cold follow the course of the magnet. I reversed the passes, and he felt it less cold, but he felt no attraction between his hand and the magnet. I then removed the cross-bar, and tried the effect with both poles alternately, but still there was no change in the effect, and decidedly no proof of attraction between his hand and the magnet. In the afternoon of the same day I desired him to look aside and hold his hat between his eyes and his hand, and observe the effects when I operated on him, whilst he could not see my proceedings. He very soon described a recurrence of the same sort of sensations as those he felt in the morning, but they speedily became more intense and extended up the arm, producing rigidity of the member. In the course of two minutes this feeling attacked the other arm, and to some extent the whole body, and he was, moreover, seized with a fit of involuntary laughter, like that of hysteria, which continued for several minutes—in fact, until I put an end to the experiment. His first remark was, "Now this expe-

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impression, as there are differences in the mental and physical constitution of the nature of the patients submitted to the processes. Some become rapidly and intensely affected, others slowly and feebly so. Such being the facts of the case, it must be obvious that no one can be at all qualified for testing such patients, who is not quite familiar with these peculiarities. He will, otherwise, be continually liable to suspect that he detects discrepancies, which arise entirely out of his own imperfect knowledge of the subject, leading him to mistake the phenomena of one stage for those which belong to another.

riment clearly proves that there must be some intimate connection between mineral magnetism and mesmerism, for I was most strangely affected, and could not possibly resist laughing during the extraordinary sensations with which my whole body was seized, as you drew the magnet over my hand and arm." I replied that I drew a very different conclusion from the experiments, as I had never used the magnet at all, nor held it, nor anything else, near to him; and that the whole proved the truth of my position as to the extraordinary power of the mind over the body, and how mental impressions could change physical action.

I operated on two other gentlemen the same day, who were much older, and with decidedly marked effects in both, though less so than in the last case. The experiment being tried with a lady of fifty-six years of age, by drawing a gold pencil-case slowly from the wrist to the finger ends, a creeping, twitching, sensation was felt, which increased until it became very unpleasant, and excited a drawing, crampy feeling in the fingers of that hand. On causing her to look aside, watch, and describe her feelings during my subsequent operations, the results were similar, and that whilst I had done nothing; and the whole, therefore, was attributable to the power of the mind in changing the physical action.

Another interesting case of a married lady, was tested in presence of her husband, as follows. I requested her to place her hand on the table, with the palm upwards, so situated as to enable her to observe the process I was about to resort to. I had previously remarked, that by my drawing something slowly over the hand, without contact, whilst the patient concentrated her attention on the process, that she would experience some peculiar sensations in consequence. I took a pair of her scissors, and drew the bowl of the handle slowly from the wrist downwards. I had only done so a few times, when she felt a creeping, chilly sensation, which was immediately followed by a spasmodic twitching of the muscles, so as to toss the hand from the table, as the members of a prepared frog are agitated when galvanised. I next desired her to place her other hand on the table, in like manner, but placed so, that by turning her head in the opposite direction, she might not see what was being done, and to watch her sensations in that hand, and tell us the result. In about the same length of time, similar phenomena were manifested, as with the other hand, although in this instance I had done nothing whatever, and was not near her hand. I now desired her to watch what happened to her hand, when I predicated that she would feel it become cold, and the result was as predicted, and vice versa, predicating that she would feel it become intensely hot, such was realized. When I desired her to think of the tip of her nose, the predicated result, either of heat or cold, was speedily realized in that part.

Another lady, twenty-eight years of age, being operated on in the same manner, whilst looking at my proceedings, in the course of half a minute, described the sensation as that of the blood rushing into the fingers; and when the motion of my pencil-case was from below, upwards, the sensation was that of the current of blood being reversed, but less rapid in its motion. On resuming the downward direction, the original feeling recurred, still more powerfully than at first. This lady being requested now to look aside, whilst I operated, realized similar sensations, and that whilst I was doing nothing.

The husband of this lady, twenty-eight and a half years of age, came into the room, shortly after the above experiment was finished. She was very desirous of my trying the effect upon him, as he was in perfect health. I requested him to extend his right arm laterally, and let it rest on a chair with the palm upwards, to turn his head in the opposite direction, so that he might not see what I was doing, and to concentrate his attention on the feelings which might arise during my process. In about half-a-minute he felt an aura like a breath of air passing along the hand; in a little after, a slight pricking, and presently a feeling passed along the arm, as far as the elbow, which he described as similar to that of being slightly electrified. All this occurred while I had been doing nothing, beyond watching what might be realized. I then desired him to tell me what he felt NOW,—speaking in such a tone of voice, as was calculated to lead him to believe I was operating in some different manner. The result was that the former sensations ceased; but, when I requested him once more to tell me what he felt now, the former sensations recurred. I then whispered to his wife, but in a tone sufficiently loud to be overheard by him, observe now, and you will find his fingers begin to draw, and his hand will become clenched,—see how the little finger begins to move, and such was the case; see the next one also going in like manner, and such effects followed; and finally, the entire hand closed firmly, with a very unpleasant drawing motion of the whole flexor-muscles of the fore-arm. I did nothing whatever to this patient until the fingers were nearly closed, when I touched the palm of his hand with the point of my finger, which caused it to close more rapidly and firmly. After it had remained so for a short time, I blew upon the hand, which dissipated the previously existing mental impression, and instantly the hand became relaxed. The high respectability and intelligence of this gentleman rendered his testimony very valuable; and especially so, as he was not only wide awake, but had never been either mesmerised, hypnotised, or so tested before.

Another gentleman, twenty-one years of age, was tried by drawing a gold pencil-case along the palm of the hand, without contact. At the fourth traction, he looking at the part and my process, he described a cold aura following the course of the pencil, then a pricking, and after a few more courses, he described it as rather a warm pricking sensation; such as that experienced from the sun's rays concentrated on the skin by a lens. By reversing the passes, from the points of the fingers towards the wrist, the aura was changed, and he described the sensation as that of forcing back the blood in the veins, when they are much distended. I then proposed to experiment on his other hand, whilst his head was held aside, when similar sensations were realized, although on this occasion I had done nothing,—the whole results having arisen from his own concentration of consciousness, changing the physical action of the part, and recalling his former association of ideas in reference to the other hand. I thereupon explained to him the law, (for he was a very acute young gentleman,) which seemed to obtain in the production of such phenomena, and desired him to satisfy himself of the fact, by concentrating his attention on the upper part of his foot, and watching the result. Here, also, he experienced similar sensations to arise, even whilst he was aware that I was doing nothing; but the effects took place less rapidly than was the case in the former experiments with his hands. It is also worthy of remark, that this gentleman experienced very severe headach to result from these experiments, which, however, I was enabled very speedily to remove by another process.

A lady thirty years of age was requested to hold out her right hand over the arm of an easy chair, whilst she turned her head to the left, to prevent her from seeing what I was doing, and to watch and describe to me the feelings she experienced in the hand during my process, which was to be performed without contact. She very soon felt a pricking in the point of the third finger, which increased in intensity, and at length extended up the arm. I then asked her how her thumb felt, and presently the same feeling was transferred to it; and when asked to attend to the middle of the forearm, in like manner the feeling was presently perceived there. All the time I had been doing nothing; the whole was the result of her own mind acting on her hand and arm. I now took the large magnet, and allowed her to watch me drawing it slowly over the hand, when the feeling was much as before, only that she felt the cold from the steel when brought very near to the skin. It was precisely the same when closed as when opened, and the same sensations occurred when the north pole alone was approximated, or the south alone, or both together. She experienced no sense of attraction between her hand and the magnet from either pole, nor from both combined. I now requested this lady to keep a steady gaze upon the poles of the large horse-shoe magnet, and tell me if she saw anything, (the room was not darkened nor was the light strong), but nothing was visible. I then told her to look steadily, and she would see flame or fire come out of the poles. In a little after this announcement she started, and said, " Now I see it, it is red; how strange my eyes feel;" and instantly she passed into the hypnotic state. This lady had been repeatedly hypnotised. I now took the opportunity of testing her as to the alleged power of the magnet to attract her hand when asleep, but, as in the other cases, the results were quite the contrary—the cold of the magnet (and of either pole alike) caused her to withdraw her hand the moment it touched her. I now requested her to tell me what she saw (she being still in the sleep.) She said she still saw the red light. I desired her to put her finger to the place where she saw it. This she declined to do, being afraid that it would burn her. I thereupon assured her that it would not burn her, upon which she pointed to the same place where the magnet was held before she went into the sleep, instead of to where it was now held, which was near to her face, but towards the opposite side of the chair. This lady does not see from under her partially closed eyelids when hypnotised, as some patients do; and the evidence her testimony affords in support of my opinion upon this subject is very conclusive, as she is a lady of very superior mental attainments, and one whose testimony merits unlimited confidence.

I beg to call particular attention to the fact, that in this latter case, as with the fifth of the vigilant cases narrated, the first experiments were tried without any magnet or other object being pointed at or drawn over them, and still the mental influence was quite sufficient to change the physical action, and produce decided and characteristic effects, where there could be no influence from without, of the nature alleged by Baron Reichenbach and the mesmerists.

A lady, upwards of fifty-six years of age, in perfect health, and wide awake, having been taken into a dark closet, and desired to look at the poles of the powerful horse-shoe magnet of nine elements, and describe what she saw, declared, after looking a considerable time, that she saw nothing. However, after I told her to look attentively, and she would see fire come out of it, she speedily saw sparks, and presently it seemed to her to burst forth, as she had witnessed an artificial representation of the volcano of Mount Vesuvius at some public gardens. Without her knowledge, I closed down the lid of the trunk which contained the magnet, but still the same appearances were described as visible. By putting leading questions, and asking her to describe what she saw from another part of the closet, (where there was nothing but bare walls,) she went on describing various shades of most brilliant coruscations and flame, according to the leading questions I had put for the purpose of changing the fundamental ideas. On repeating the experiments, similar results were repeatedly realized by this patient. On taking this lady into the said closet after the magnet had been removed to another part of the house, she still perceived the same visible appearances of light and flame when there was nothing but the bare walls to produce them ; and, two weeks after the magnet was removed, when she went into the closet by herself, the mere association of ideas was sufficient to cause her to realize a visible representation of the same light and flames. Indeed such had been the case with her on entering the closet ever since the first few times she saw the light and flames. In like manner, when she was made to touch the poles of the magnet when wide awake, no manifestations of attraction took place between her hand and the magnet, but the moment the idea was suggested that she would be held fast by its powerful attraction, so that she would be utterly unable to separate her hands from it, such result was realized ; and, on separating it, by the suggestion of a new idea, and causing her to touch the other pole in like manner, predicating that it would exert no attractive power for the fingers or hand, such negative effects were at once manifested. I know this lady was incapable of trying to deceive myself, or others present ; but she was self-deceived and spell-bound by the predominance of a pre-conceived idea, and was not less surprised at the varying powers of the instrument than others who witnessed the results.

Similar phenomena were manifested by this patient when she was requested to place a finger or hand in contact with a table, or any other inanimate object ;—the results being either negative, or irresistibly attractive, or repulsive, according to the ideas suggested ; and that without any passes or manipulations on any part. Moreover, these results occurred whilst I was willing the very opposite effects might arise from those she had heard me predicate. The results always corresponded with the mental impression or belief of the patient, and might have been just as readily suggested by a pass or silent signal, to which she had attached a particular idea, as by vocal enunciation.

In like manner several other patients whom I took into the dark closet could see nothing, until told to look steadily at a certain point and they would see flame and light of varying colours, proceeding from it, which predictions were speedily realized, whilst they were wide awake, and with nothing but bare walls towards which to direct their eyes. Not only so, but I have, moreover, ascertained, that, even in broad day-light, a strong mental impression is adequate to produce such delusions with certain individuals of a highly imaginative and concentrative turn of mind. This fact was beautifully illustrated in the case of a gentleman, twenty-four years of age, (who had suffered severely from epilepsy, for eleven years, notwithstanding the persevering use of medicine of various sorts, prescribed by many of the most able members of the profession, but who is recovering very satisfactorily under the hypnotic treatment,) when taken into the above closet and tested as the latter, He likewise saw nothing till I suggested that he would see flame and light, after which predication he very speedily saw them accordingly, not merely where the magnet was, but also from other parts of the apartment. Now, this patient, and the last two referred to, when taken into the closet after the magnet had been a long time removed to a distant part of the house, still saw the flames, and changing colours as before —a clear proof that the whole was a mental delusion arising from an excited imagination, on the point under consideration, changing physical action. The same gentleman being made to look at the point of a piece of brass wire, could be made to imagine that he saw any sort of flame or colour indicated issuing from it, even in broad day-light; and when made to touch it with a finger, and then told he would find it impossible to draw it away, the mere idea was sufficient to paralyze his volition, the whole muscles became rigid and he looked with astonishment at his condition; but the moment I said now the attraction is gone, and his hand will separate, such results followed. Moreover, now that his finger was a little withdrawn, by simply saying confidently that it would now be found that he could not touch the wire, as it would repel him, the idea once more paralyzed his volition, and he again manifested his incapacity, and in spite of his anxious, but misdirected efforts, there he remained fixed as a statue. On hinting that now the influence was suspended, the hand and arm became limber, when I told another person watching the experiment that now he would find the hand irresistibly drawn to the wire; and such result was presently manifested. No one had touched this wire for hours; it was merely a piece of bent brass wire which was lying loosely and projecting from the chimney-piece. This power seems to have been understood by Virgil, when he said—

"Possunt, quia posse videntur."

In like manner having intimated to a friend the remarkable vividness of this patient's imagination, implicit belief, and credulity, which rendered him liable to believe that he had an ocular perception of an external change, according to whatever idea might be suggested to him by others, I requested this friend, when we went into the room, to look at the end of the above wire at the same time with the patient, and that the former should pretend to me, when asked what coloured flame she saw emanating from it, to give a new idea at each inquiry. By this mode the patient caught the ideas suggested, having no notion that he was deluded in the way indicated. He left with the full conviction of the physical reality of all he had seen and described; and he has manifested like phenomena as frequently as he has been so tested.

I have detailed the above case so much at length, because it is a very good type of a class of patients to be met with, who readily become the dupes of suggestions, in the manner presently to be explained, without the least desire to deceive others, or the most distant idea that they are themselves deceived. I have proved all I have advanced by so many concurrent examples, with individuals of the utmost probity and competency to describe their feelings, that there can be no doubt of the facts.

But not only may patients, in the waking state, be made to believe that they see various forms and colours, and perceive variable sensible impressions and irresistible powers, drawing, repelling, or paralyzing them, from a strong mental impression changing the physical action of the organ or part usually engaged in the normal manifestation of such function; but I have, moreover, ascertained that the same influence may be realized in respect to sound, smell, taste, heat, and cold,—so that suggested ideas and concentration of consciousness are competent, with some individuals, to excite ideas, not merely of hearing vague sounds, but particular tunes, the smell of particular odours, and to discriminate particular tastes, and feel heat or cold. All this I have proved may be realized with some excitable subjects when they were wide awake; and when there was neither actual sound, nor odour, nor taste, in the situations and substances to which they were referred; and by merely asking what tune, what colour, what animal, or what substance, they perceived now, I clearly proved that ideas were thus excited in the minds of subjects totally different from those existing in my own at the time. The subjects with whom I made these experiments were worthy of implicit credit as to their integrity in describing their feelings and belief; and the whole results, therefore, are attributable to the remarkable reciprocal actions of the mind and body on each other, to which I have so often referred.* Indeed, one of the most beautiful examples I have

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* For remarkable examples of the extent, to which mental impression is capable of changing physical action, I beg to refer to my papers in the Medical Times for December 1844, and January and February 1845. It is particularly gratifying to be able to refer, in confirmation of most of the views I advocate, to Dr Holland's "Medical Notes and Reflections,"—one of the most masterly works I ever read, which I had the pleasure of perusing for the first time in June 1845.

Ten days ago I had the gratification to receive from this gentleman a letter, expressing his entire concurrence with my published views of mesmeric phenomena generally; and in respect to my comments and experiments in opposition to the views of Reichenbach, he considers them so satisfactory and conclusive that "no further contradiction is necessary." I have also had similar testimony in favour of my views from many other gentlemen of high talents and attainments.

Farazi, the Persian necromancer's answer to Firuz Shah, is also quite corroborative of my views. When asked how it was possible for him to bring a person had of these "vigilant phenomena," in respect to all the senses, occurred in the case of a gentleman of high classical and mathematical attainments, as well as in general science. He had seen no experiments of the sort before I tested himself.* On finishing my round of experiments with him, he begged of me to explain the rationale of what had occurred. I requested him to read what I had written on the subject, which he perused with great attention; after which he expressed himself perfectly satisfied that I had hit upon the true solution of the problem. Indeed, he was so kind as to authorise me to refer any one to him for a confirmation of the rationale I had given of the phenomena, as experienced by him in his own person when wide awake, and in the bright light of day.

It is stated by Baron Reichenbach that the sensitive can uniformly detect water which has had the magnetic currrent passed through it, by having an open magnet brought over and in contact with the opposite sides of the glass in which the water is contained. I have tried this experiment in such a manner as was calculated to guard against various sources of fallacy which I knew were likely to mislead the unwary; such as the varied tone

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immediately to that spot, who was known to be at the time 800 or 900 miles distant, he replied that "HE COULD NOT BRING HER IN PERSON, BUT COULD PRODUCE SUCH A LIKENESS OF HER THAT HE COULD SWEAR TO HER BEING HIS OWN WIFE."—Late Right Hon. Sir Gore Ousley's "Biographical Notices of Persian Poets."

* When testing this gentleman, by requesting him to listen attentively and tell me what tune he heard from an object held near to, but not in close contact with his ear (it was merely a pencil case), he named a well known *cheerful* tune. Without saying a single word, I applied the finger and thumb of my other hand so as slightly to corrugate the integuments of the forehead, when he instantly said, "the tune is changed; it is Roslin Castle now." On my separating the fingers and thumb, so as to expand the integuments, he answered, that the music was lively again. These changes took place from the grave to the gay and *vice versa*, as often as I made the corresponding changes on the integuments. If I named any particular tune which he would hear, without touching him, he immediately thought he heard the tune I had named.

In like manner having asked him to tell me what *smell* he felt when the pencil case was held to the nose, (it was a gold pencil case, and therefore could have had neither smell nor taste), he named an odour, but the moment I corrugated the integuments of the forehead, as before named, it changed for something disagreeable, and *vice versa* as often as such changes in the "Anatomy of Expression" were made. Similar delusions took place likewise in respect to taste, when he was requested to tell me what *taste* the pencil case had under different circumstances, being agreeable with the expanded and elevated state of the integuments, and the reverse when they were contracted or depressed. Visual spectra, in like manner, could be rendered agreeable or disagreeable, bright or sombre, according to similar changes in the physical frame. The philosophical explanation of all this I have entered into pretty fully in my forthcoming treatise on hypnotism or nervous sleep; and it is on these principles that I have endeavoured to account, in a very simple and natural manner, for the excitation of the phenomena of mesmero-phrenology as they have been called, by touching certain parts of the scalp during mesmeric sleep, where all sources of fallacy are guarded against from auricular suggestion or previous knowledge of phrenology. According to my apprehension, exciting these phenomena in such manner, many of which are extremely interesting and beautiful, neither proves nor disproves the doctrines of phrenology, but just leaves that subject where we found it.

of voice or look of the experimentalist, when investigating the subject, and trying to elicit from the patient his candid opinion as to the qualities of each glass of water. The results were, that none of the patients tried could detect either by smell, taste, or any other sensible effect, any difference between the magnetised and the unmagnetised water. At each trial I made use of four similar glasses, filled to the same height, two of which, by the contact to the glass of a powerful magnet for a minute or two, were magnetised, the other two not magnetised. The patients were all first tested in this way when awake, and then when asleep. Magnetising by passes, or breathing on the water, I should not consider a fair experiment, and for this reason, that the halitus of the breath, or from the skin, might very readily be detected by a highly sensitive patient, by smell, and probably also by taste. I have met with a patient who could very readily detect water so treated from that which had not, and I ascertained that she did so by smell.

The extreme acuteness of the organs of special sense, under certain circumstances, is well illustrated in certain states of disease, and by what is realized in the feats of some savage tribes. They appear quite as exalted as anything I have met with, in the hypnotic or mesmeric state; and which has been sufficient to enable me to account, on natural principles, for what others have attributed to some new faculty or sense developed by their processes, and distinct from the ordinary organs of sense.

However, I have never met with any case of accurate thought-reading, where the patients could generally and accurately interpret my unexpressed thoughts and desires regarding him, without some sensible sign to indicate my ideas to him; but, as already stated, I feel confident that there are many, who, from their excited and concentrated state of mind, and quickened senses, will catch ideas far more quickly than in the ordinary condition, who may be thus imposed upon, and excited to act by means of impressions received through the senses, which would entirely escape them in the ordinary condition.

In consequence of the statement made in the third section, of the tranquillising effect of causing highly sensitive patients to lie with the head towards the north, and the extreme discomfort of other directions of the body, in reference to the magnetic meridian, I had a patient who had been long an extreme sufferer, and in whose welfare I felt the deepest interest, to submit to the trouble of such a change in the position of her bed, but, I am sorry to say, it was followed by no beneficial effect.

I am perfectly aware that the results of a single case are not competent to refute what has been alleged, as proved by a number of others. However, I have stated my fact on the point;

and, whilst I readily admit my firm conviction that, with sensitive patients, who, from whatever cause they may have imbibed notions that certain directions of the body, either when asleep or awake, might be followed by feelings of physical as well as mental discomfort, the mere idea would be adequate to the production of such effects ; still, I do not believe there is any adequate physical cause for such results, merely as regards the polarity of the earth and human frame. The following are the grounds for this opinion. The constituent parts of the human frame are all dia-magnetic, that is, under the magnetic influence, they have a tendency to assume the equatorial axis, instead of the polar direction ; and it therefore appears to me extremely improbable that a combination of dia-magnetics should, merely in consequence of their combination, become invested with the very opposite qualities of the constituent parts. To say the least of it, this would be at variance with all which has yet been ascertained of such powers on the inanimate, physical, and chemical world. It therefore appears to me, that the peculiarities referred to by Baron Reichenbach, under this head, are more likely to have arisen from a mental than a physical cause.

A few days after the above paragraph was written, I read it to a most intelligent friend, who told me, that she had just received a letter from her brother, who had been attending some lectures by Professor Faraday, and kindly offered to send me an extract from his letter, corroborative of what I had read to her. The following is the extract which I received the following morning, and is particularly gratifying, coming as it does from the very highest authority we have on this subject. "I have been greatly interested lately with a course of lectures by Faraday on Electricity and Magnetism, which, I am sorry to say, is just concluded. You will no doubt be gratified to learn that, in common with the whole human race, you are a magnet ! with this peculiarity only, that if you were suspended by the middle, you would point East and West, instead of North and South—with which interesting fact, I leave you to your meditations !"

That some of Baron Reichenbach's patients should have manifested the power of seeing luminous emanations in the dark, over the graves of the dead, more especially of those recently buried, I think admits of a more probable explanation than that propounded by the Baron. We know that during certain stages of decay of vegetable and animal matter, a phosphorescent appearance is manifested in the dark to ordinary vision, and, of course, it must be still more so to the highly sensitive. But there is another result of such organic decay still more powerfully felt by all, namely, its disagreeable odour. Few can escape the recognition of this, even in the bright light of day. Again : we are so much in the habit of associating visible or tangible forms with impressions which are conveyed to our minds through any of the organs of sense, that the blind are in the habit of expressing themselves as seeing such and such persons, whom they recognise approaching, merely by the sound of their voices, or tread of their steps, and so on.

In like manner a smell of any sort suggests corresponding ideas; and I have found hypnotic patients who recognised individuals of their acquaintance quite readily by smell, who always said, when asked how they knew who was present, " I see him or her," and, when the nostrils were stopped, thought the party had gone away. The sympathy which exists between the eyes and the nose is well proved by a strong light producing sneezing; and the converse of this is very natural, that a strong impression on the olfactory nerves might excite in the mind of a highly sensitive and imaginative person the idea of some visible appearance. This will account for the ghost-story quite as readily as the theory of the Baron, and will be found more generally available, as few people are so constituted as to be able to pass through such places without being painfully reminded, through their olfactory organs, that they are treading on the mortal remains of friends departed.

But I may be met with the objection to my arguments by an appeal which has been made to the Daguerreotype, (a purely physical fact),—which could not be influenced by mind,—for an experiment has been recorded by Baron Reichenbach, where, by the mere exposure of a sensitive plate in a box with a magnet, all light being excluded, an impression had been made, as if it had been exposed to the full influence of light, which did not happen, when another plate was so confined without the magnet. It appeared to me that such an important deduction ought not to have been arrived at, from one or two experiments of such a ticklish nature; and more especially so, when the iodised plates were so long kept before being exposed to the mercurial vapour; for it was natural to expect some chemical change might take place with them irrespective of light. I have repeated these experiments with nine plates, prepared at different times with the greatest care by Mr Akers, the patentee of the Manchester photographic gallery, (one of the most correct professors of his art,) and the experiments varied so as to guard against error, by mutual correction. The experiments have been tried by close and free exposure of three plates to the poles of a powerful horse-shoe magnet, (originally of 80 lbs. lifting power, but somewhat reduced from use,) and of other three similarly situated, only with two sheets of black paper interposed so as to intercept the rays of light, if any were emitted from the magnetic poles, and another was enclosed in a box at a distance from the magnet, and all kept from

66 to 74 hours; but in no instance was there any appearance of them having been acted on by light,—the only change being such chemical changes as generally arise from keeping prepared plates for some time before being exposed to mercury. The latter process was also performed by Mr Akers for the purpose of insuring greater accuracy from his daily practice of the art. Two other plates were enclosed in a camera and exposed at such a distance as must have given a picture of the poles of the magnet, and flames issuing from them—for the camera was adjusted to 4½ times the distance of ordinary light, and one left 66 the other 35½ hours—but no such indications manifested themselves. I think, therefore, that I am warranted to conclude that no light, capable of affecting an iodised plate, is given out at the poles of magnets, as alleged from the experiments referred to by Baron Reichenbach.*

Having thus far investigated Baron Reichenbach's speculations, and proved the major propositions to be erroneous, as respects proving the phenomena he refers to as resulting from a "new imponderable," I consider it unnecessary to prosecute the subject in the minor details. I may, however, remark, that I can readily imagine, from the exalted sensibility of particular individuals, and their highly concentrative state of mind, that they may be competent to detect electric, calorific, and physical qualities of objects, which would escape the observations of themselves and others, when in the ordinary and less sensitive condition. And, again, whilst I deny that we have any satisfactory proof of this new imponderable, passing from the operator to the patient, as alleged, during the processes of the mesmerists, still I readily admit that the operator, for the reasons already assigned, of mental impression changing physical action, is very likely to feel an aura in his finger ends, the position, action, and mental direction, all tending to excite in them turgescence and increased sensibility. I can also easily conceive, that the passes, through their physical, electrical, and other effects, tending to draw the patient's attention to particular parts, thus exciting their functions, and withdrawing it from others, thereby depressing their functions, and especially if the patient expects such results to ensue from such processes, are very likely to be followed by such results, irrespective of any special influence being transmitted from the operator to the patient, of the nature alleged by Baron Reichenbach and others.

The mesmerists, who have entertained the notion that the sleep and other phenomena occurring during their processes arose from a magnetic fluid, or some special influence issuing from their persons, and passing into that of the patient, of course hail these speculations of Baron Reichenbach as demonstrative proof of the correctness of their theory of mesmerism. Some of their patients having declared that they saw blue or variegated sparks passing from the fingers of the operator, when making passes in mesmerising a patient, water, or other inanimate object; or that they saw a blue fluid, or one of still brighter hue, issuing from the eyes of the operator and darting towards them, when gazing fixedly for the purpose of mesmerising; or that they perceived a luminous halo surrounding the head of a person engaged in deep thought, must have felt gratified by the announcement, considering it well calculated to confirm in their minds, the notion of the physical entity of the agent. But, when those patients have further described that when the operator has been engaged in silently willing, for example, that a patient should be made to come towards him, there is seen by the patient, a stream of something passing from the head of the operator to that of the patient, like a rope, and that he not only sees this, but feels its influence as a rope, or innumerable threads drawing him irresistibly towards the operator, the entity must now appear fully confirmed. But again, when some of these subjects become, at length, so accomplished and susceptible as to be able to perceive these visible appearances in the wide waking state, it cannot be wondered at that those whose long cherished opinions lay in that direction, should jump to the conclusion that now the point is fully and satisfactorily settled. Under such a combination of circumstances, individuals may be excused for overlooking what to others is a very obvious source of fallacy,—viz. the tendency of a continued fixed gaze to confuse the vision, and of a fixed idea and expectation to confuse and bewilder the mind.

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* Mr Dancer, the optician, kindly lent me apparatus, and saw and approved of my arrangements, as well adapted for the purpose.

It is well known that the undue excitement of an organ disturbs the normal functions of such organ; and that in a state of fever, delirium tremens, and various morbid states of the nervous system, and even in the healthy state during dreaming, the patient imagines that he feels, and sees, or hears all sorts of forms, colours, sounds, &c. We do not, on that account, subscribe our belief in the reality of the objects, or peculiar attributes he has seen, heard, or felt, or the imaginary feats he has accomplished. In this case we estimate the phenomena as mental and morbid delusions; and such I apprehend ought to be our conclusion in reference to these extraordinary and startling allegations.

Inasmuch as patients can throw themselves into the nervous sleep, and manifest all the usual phenomena of mesmerism, through their own unaided efforts, as I have so repeatedly proved by causing them to maintain a steady fixed gaze at any point, concentrating their whole mental energies on the idea of the object looked at; or that the same may arise by the patient looking at the point of his own finger, or as the Magi of Persia and Jogi of India have practised for the last 2400 years, for religious purposes, throwing themselves into their ecstatic trances by each maintaining a steady fixed gaze at the tip of his own nose; it is obvious that there is no need for an exoteric influence to produce the phenomena of mesmerism. The agency may be entirely personal or subjective, and in such cases as I have illustrated by extracts from Ward's History of the Hindoos and the Dabistan, through certain associations of ideas, such self-mesmerised patients can see and imagine as great, or indeed far greater wonders than are recounted by our most successful mesmerists of modern times, with the additional aid of their alleged mesmeric fluid. In proof of this I beg to refer to my paper in the Medical Times, page 272, vol. xi. The great object in all these processes, is to induce a habit of abstraction or concentration of attention, in which the subject is entirely absorbed with one idea, or train of ideas, whilst he is unconscious of, or indifferently conscious to, every other object, purpose, or action.*

I had long been familiar with the fact, that during a certain stage of hypnotism, patients may be made to give various manifestations, or declarations of their feelings and emotions, according to previously existing ideas, or suggestions imparted to them during the sleep; and, moreover, that such associations once formed, were liable to recur ever after, under a similar combination of circumstances. As occurs in ordinary dreaming, they seem generally at once to adopt the idea as a reality, without taking the trouble of reasoning on the subject as to the probability of such ideas being only imaginary; and their extreme mobility at a certain stage of the hypnotic state, renders them prompt with their corresponding physical response. In proof of this, and how readily those inattentive to these facts may misapprehend what they see realized in such cases, I beg to submit the following interesting illustration. When in London lately, I had the pleasure of calling upon an eminent and excellent physician, who is in the habit of using mesmerism in his practice, in suitable cases, just as he uses any other remedy. He spoke of the extraordinary effects which he had experienced from the use of magnets applied during the mesmeric state, and kindly offered to illustrate the fact on a patient who had been asleep all the time I was in the room, and in that stage, during which I felt assured she could overhear every word of our conversation. He told me, that when he put the magnet into her hands, it would produce catalepsy of the hands and arms, and such was the result. He wafted the hands and the catalepsy ceased. He said that a mere touch of the magnet on a limb would stiffen it, and such he proved to be the fact.

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* From careful observation, I feel convinced that the principal differences between nervous and common sleep, consist in the state or condition of the mind, and the physical effects which flow therefrom. In proof of this and the proposition above referred to, I beg to refer to the fact that I never yet succeeded in hypnotising an idiot; for the very obvious reason that there was not a sufficient amount of intelligence to enable me to fix their attention. However, I have frequently succeeded in inducing the sleep in ordinary cases of insanity, and monomania, and in several instances with decided advantage. In some by restoring refreshing sleep which had been long absent, in others by exciting in their minds during the sleep, antagonist ideas to those which constituted their chief delusion. Fixing the attention through visual sensation, as specially indicated in my little treatise on Hypnotism, is the most rapid and certain mode of inducing the state of nervous sleep, but the patient may be affected, as the blind are, by closing the eyes and keeping his mind fixed on some imaginary object or idea. After the impressibility has been stamped on subjects which are naturally highly susceptible, they become liable to be affected entirely through mental impression, belief, and habit,—that is to say, they will become affected from any visible process which they believe intended and capable of producing the effect, or even where no process whatever is in operation, if they only imagine something is being done at a distance to throw them into the sleep, they will become affected through the mere force of this mental impression and belief. This seems to be a grand source of fallacy with many who allege that they can affect patients at a distance, through mere volition, or secret passes, occasional coincidences being exalted by them into a positive law.

I now told him, that I had got a little instrument in my pocket, which, although far less than his, I felt assured would prove quite as powerful, and I offered to prove this by operating on the same patient, whom I had never seen before, and who was in the mesmeric state when I entered the room. My instrument was about three inches long, the thickness of a quill, with a ring attached to the end of it. I told him that when put into her hands, he would find it catalepsize both hands and arms as his had done, and such was the result. Having reduced this by wafting, I took my instrument from her, and again returned it, in another position, and told him it would now have the very reverse effect—that she would not be able to hold it, and that although I closed her hands on it, they would open, and that it would drop out of them, and such was the case,—to the great surprise of my worthy friend, who now desired to be informed what I had done to the instrument to invest it with this new and opposite power. This I declined doing for the present; but I promised to do so, when he had seen some further proofs of its remarkable powers. I now told him that a touch with it on either extremity would cause the extremity to rise and become cataleptic, and such was the result; that a second touch on the same point would reduce the rigidity, and cause it to fall, and such again was proved to be the fact. After a variety of other experiments, every one of which proved precisely as I had predicted, she was aroused. I now applied the ring of my instrument on the third finger of the right hand, from which it was suspended, and told the doctor, that when it was so suspended, it would send her to sleep. To this he replied, " it never will;" but I again told him that I felt confident that it would send her to sleep. We then were silent, and very speedily she was once more asleep. Having aroused her, I put the instrument on the second finger of her left hand, and told the doctor that it would be found she could not go to sleep, when it was placed there. He said he thought she would, and he sat steadily gazing at her, but I said firmly and confidently that she would not. After a considerable time the doctor asked her if she did not feel sleepy, to which she replied "not at all;" could you rise and walk? when she told him she could. I then requested her to look at the point of the fore-finger of her right hand, which I told the doctor would send her to sleep, and such was the result; and, after being aroused, I desired her to keep a steady gaze at the nail of the thumb of the left hand, which would send her to sleep in like manner, and such proved to be the fact.*

Having repaired to another room, I explained to the doctor the real nature and powers of my little and apparently magical instrument,—that it was nothing more than my portmanteau-key and ring, and that what had imparted to it such apparently varied powers was merely the predictions which the patient had overheard me make to him, acting upon her in the peculiar state of the nervous sleep, as irresistible impulses to be affected, according to the results she had heard me predict. Had I predicted that she would see any flame, or colour, or form, or substance, animate or inanimate, I know from experience that such would have been realized, and responded to, by her; and that, not from any desire on her part to impose upon others, but because she was self-deceived, the vividness of her imagination in that state, inducing her to believe as real, what were only the figments of fancy, suggested to her mind by the remarks of others. The power of suggestions of this sort also, in paralyzing or energising muscular power is truly astounding; and may all arise in perfect good faith with almost all patients who have passed into the second conscious state, and with some, during the first conscious stage; and with some weak-minded, or highly imaginative or credulous and concentrative people, even in the waking condition. The latter constitutes that class of subjects who manifest what are called the "vigilant phenomena." The true cause of these "vigilant phenomena" is not a physical influence from without, but a mental delusion from within, which paralyzes their reason, and independent volition, so that for the time being they are mere puppets in the hands of another person by whom they are irresistibly controlled, so that they can only see or, hear, or taste, or feel, or act, in accordance with his will and direction.* They have their whole attention fixed on what may be said or signified by this alleged superior power, and consequently perceive impressions through the excited state of the organs of sense, called into operation, which they could not perceive in their ordinary condition; and this sort of clairvoyance or thought-reading, the mesmerists attribute to some special influence, such as the new imponderable of Reichenbach. So soon as such patients can be made to believe that, in the waking state, the evidence of their senses is more trust-worthy than mere ideal or suggested impressions; and that they can really exercise their own independent powers in opposition to the alleged power of the will of another, through his auricular suggestions or passes, and other manœuvres, it will instantly be found that the spell is broken, and that rational beings can no longer be magnetically tied together, or to chairs, or tables, or the floor; or made to see an object of every colour and hue, or metamorphosed into every nature, form, or creature, the operator may incline and indite. It may have been interesting enough to have demonstrated that the human mind could be so subjugated and controlled; but, as I have formerly said, I do not consider the continual repetition of such experiments in the waking state, as at all proper, or free from the danger of throwing the faculties of the mind of such subjects into a permanently morbid condition.

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* The doctor here alleged that I had sent the patient to sleep by my *will*. This assertion, however, I was enabled to refute; for I had been sufficiently *clairvoyant* to foresee what he would suppose, and to meet this difficulty had *willed* that she should *not* go to sleep.

It would be inconsistent with the scope of this paper to enter into an elaborate detail of my views as to the philosophical explanation of the modes of exciting and varying certain trains of ideas, and their consequent manifestations during the nervous sleep. The inquiry is not only curious, but also one of great interest, in respect to the power of the mind in controlling physical action, and of physical impressions in reacting on the mind. I have given a pretty ample discussion on these topics in a second edition of my little work on hypnotism, now preparing for the press, to which I beg leave to refer those who feel desirous of prosecuting the inquiry, and particularly those who desire to do so for therapeutic purposes.

In conclusion, I beg particularly to remark, that, whilst my experiments and observations are opposed to the theoretical notions of Baron Reichenbach and the mesmerists, in all the more important points, they directly confirm the reality of the facts, as to the power which we possess of artificially producing certain phenomena by certain processes; as also of intensifying effects which arise in a minor degree, spontaneously, or by the patient's own unaided efforts. They allege that the exciting cause is the impulsion into the body of the patient from without of a portion of this new force; whilst I attribute it to a subjective or personal influence, namely, that of the mind and body of the patient acting and reacting on each other in a particular manner, from an intense concentration of consciousness on one idea, or train of ideas, which may, to a certain extent, be controlled and directed by others. The latter power, however, merely arises from the mental and physical impressions producing still greater concentration of the patient's attention in a particular direction; that is to say, by concentrating their attention to the point over which they see anything drawn, or upon which a mechanical, calorific, frigorific, or electric impression is made, whereby a greater supply of nervous influence, blood, and vital action, is drawn to the part from the physical and mental resources of the patient himself, and not from the person or substance exciting those physical impressions. They enable the patient more effectually to concentrate his own vital powers, and thus to energise function; on the same principle as a patient afflicted with anæsthesia, or loss of feeling, is able to hold an object in his hand whilst he looks at it, but will allow it to drop when his eyes are averted.

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* Their minds are entirely engaged with a single idea, or train of ideas; actively alive to all which bears upon this, but, for the time being, dead to all else, as we continually see verified in cases of deep mental abstraction, and even during sleep, when the slightest impression bearing on certain points may be perceived, whilst intense impressions of any other relation may pass unheeded.

It is worthy of particular remark, that my researches prove the power of concentration of attention, as not only capable of changing physical action, so as to make some patients, in the wide waking state, imagine that they see and feel from an external influence what is due entirely to an internal or mental cause; but I have extended the researches, so as to prove, 'that the same law obtains in respect to all the other organs of special sense, and different functions of the body. My theoretical views, therefore, instead of diminishing, rather enhance the value of this power as a means of cure. They strikingly prove how much may be achieved by proper attention to, and direction of, this power of the human mind over the physical frame, and *vice versa*, in meliorating the ills which flesh is heir to. I beg further to remark, in support of my views, that in the experiments of Baron Reichenbach, and the mesmerists generally, all which I have endeavoured to prove as requisite for the production of the phenomena referred to, is necessarily brought into play during their processes, in super-addition to their alleged mesmeric fluid, or new force: of the latter, therefore, under such circumstances, I maintain that we have, as yet, had no direct and satisfactory proof; and it is unphilosophical to attribute to a new and extraneous force what can be readily accounted for from the independent physical and psychical powers of the patient, which must necessarily be in active operation, along with their processes, and alleged new imponderable.

The results of my experiments, moreover, satisfactorily prove the unity of the mind; and the remarkable power of the soul over the body.

3 *St Peter's Square*,
Manchester, *August* 18, 1846.

ART. V.—*An Experimental Inquiry into the Functions of the Ophthalmic Ganglion; with an application of the inferences obtained to the physiology of the Ganglionic System in general.* By C. RADCLYFFE HALL, M.D., M.R.C.S. Eng., &c. Part Third. With Microscopic Illustrations.

The ganglionic nerves are the organic nerves for all the tissues, excepting, probably, the nervous and the muscular.—We have proof sufficient that the nervous centres for intellect, sensation, and muscular motion, exist in the cerebro-spinal system. All the nervous centres, except one, have functions correctly assigned to them. All the known functions of the nervous system, except one, have their nervous centres ascertained. The exceptional function, (viz. the control and harmonization of the many and various molecular actions classed as the organic processes,) must belong to the exceptional nervous centre, (viz. the ganglia in the aggregate).

In this syllogism, however, as in the remarks which preceded, the following important physiological points are taken for granted. (1.) That the different portions of the cerebro-spinal axis really have the functions which physiologists *agree* in assigning to them. (2.) That the ganglia are in no way directly essential to the discharge of these functions. (3.) That the superintendence of the organic processes is a function of some nervous centre. (4.) That the ganglia are anatomical nervous centres, and taken collectively may be viewed as one great nervous centre, just as the numerous spinal segments are considered to constitute the spinal centre.

Of these, the first will be admitted, and the second and third have already been noticed. The fourth, although now very generally allowed, is by no means so firmly established as to render its discussion superfluous.

To decide whether a ganglion possess the anatomical characteristics of a nervous centre, we must ascertain the exact arrangement of nerve-tubes and cells in parts acknowledged to be sources of nervous power. The difficulty of doing this is due less to the minuteness of the objects, than to their great delicacy and softness, rendering the more common modes of preparation for microscopic examination unsuitable. After failure in repeated attempts on portions of nervous tissue treated with alcohol, diluted acetic acid, or saline liquids, the following plan was pursued with more success. The thinnest possible slice was carefully removed on the edge of a sharp razor from some portion of the nervous system of an animal *just killed;* always, in the higher classes, whilst the body was quite warm. This slice was placed on a piece of thin glass, and examined sometimes uncovered, sometimes covered by another piece of microscopic glass. To prevent the close adhesion of the nervous slice to the glasses, which occurs when there is great difference of temperature, and causes any movement of the glasses upon each other to distort the outline of the objects between them, the pieces of glass were previously dipped into warm water and then wiped quite dry. Occasionally, by gently moving the upper glass upon the lower one, appearances were made out with greater distinctness, and observations, which would otherwise have been erroneous, corrected. Isolated objects displaced around the margin of the rest often presented a well defined outline. Nervous tissue removed before the animal heat is dissipated, for a short time remains more translucent and cohesive, and may be compressed with less separation of its constituent parts than that which by delay has been allowed to "set" before examination. Still, even when quite fresh, if suddenly and forcibly squeezed between the glasses, too considerable a disruption ensues for the observation to prove satisfactory.

To ascertain the relative size of the different objects without measurement, the same power of 300 lin. diam. was used invariably. The figures cannot be trusted as representing the *actual* size, but I believe the relative proportion of the objects is tolerably correct. The greatest care was taken to give the outline of every object with accuracy.

A portion of cerebrum, cerebellum, spinal chord, spinal ganglion, or sympathetic ganglion thus examimed, furnished the following objects;

1. A straw-coloured oil-like fluid, forming, under compression, a liquid boundary around all the rest.
2. Semi-opaque white cylindrical nerve-tubes; largest in calibre.
3. Translucent tubules, not varicose; smallest in size.
4. Translucent varicose tubes; intermediate in size.
5. Regular-shaped cells, more or less spherical or ovoid; some nucleated; some without visible nuclei; of all sizes.
6. Irregular-shaped cells with projecting processes, (caudate vesicles).
7. Occasionally, cells with pigment spots.

The white nerve-tubes appear to be almost uniform in size where they leave the sphere of the grey matter, as at the margin of a ganglion, or where the grey joins the medullary matter in the encephalon; also, whenever they can be distinctly traced through all the grey matter of the object under examination. Some of these white tubes, however, in the midst of the nerve-vesicles, become manifestly smaller in calibre, and more delicate in their walls; these are either lost to view amongst the grey cells, or otherwise are seen to be continuous with the varicose tubes. This direct continuity of the straight with the varicose nerve-tubes is not always to be readily made out, owing chiefly to the readiness with which the latter break under manipulation.

The varicose tubes are smaller in calibre in the undilated portions than the straight nerve-tubes, and present, at distances somewhat equal, more frequently otherwise, flask-like dilatations resembling in outline a number of small eggs strung on a string, or a portion of the *Fucus vesiculosus.* The dilatations are often precisely similar in size on the same tube; but not always so. In shape ovoid, their long axis corresponds with that of the tube. Ehrenberg, who first described these under the name of articulated tubes, is understood to have renounced his first opinion that the arrangement is normal, and to now consider the appearance as the result of either *post mortem* change or of manipulation. Even in this case, it has been justly remarked,* the proneness to assume the varicose disposition is a distinguishing characteristic of these tubes.

The following reasons induce us to doubt whether Ehrenberg was not most correct in his original opinion. As these varicose tubes can be seen immediately after death, they are not the result of decomposition; as they can be rendered visible, particularly in the spinal chord, by mere scratching with the point of a fine needle, they are not produced by compression; as smaller and probably more delicate tubes than they, and larger and probably less delicate tubes, maintain a straight cylindrical appearance under precisely the same circumstances as to exposure and manipulation, the varicose appearance cannot be due to greater or less delicacy of texture; as there is no thinning perceptible in the walls of the dilated portion at its greatest circumference, the enlargement cannot be due to mere expansion of the tube from pressure; moreover, by the compression of a varicose tube accidentally isolated, we occasionally produce an irregular-shaped tube, very different from the regular-shaped varicose one; lastly, as Ehrenberg himself remarked, the appearance cannot be due to mere elasticity, since stretching one of the tubes, (which may sometimes be managed rather by accident than design,) will break it, but not cause any alteration in its varicosity.

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* British and Foreign Medical Review.

Remak ascribes the varicose appearance to the contraction of a longitudinal central band in the interior of the nerve-tube, but does not explain how this can occasion such regular and uniform dilatations as we actually find. Any alteration so originating would cause an irregular and puckered appearance only. Ehrenberg considered that the nerve-tubes were filled with soft medullary matter, and that the articulated appearance was restricted to the nerve-tubes appropriated to sensation. Remak goes further, and attempts to distinguish sensational and motory nerve-tubes from each other, by anatomical characteristics. According to him, a sensational nerve-tube is minute, has its walls excessively thin, and appears flat, transparent, and varicose ; a motor nerve-tube is larger, thicker, has its walls more resistant, and appears opaque, cylindrical, and merely rugous, not varicose. Remak is peculiar in holding these opinions, which are quite unsupported by microscopical observation. The varicose tubes are found wherever grey nerve-cells exist, in the convolutions of the brain and cerebellum, in the centric and basic grey masses of the encephalon, in the olfactory, optic, and auditory nerves,—which are rather processes of brain than strictly nerves,—in the spinal chord, and in the ganglia. They are never found in the purely white part of the nervous centres, nor in the nerves, with the above exceptions.

In addition to the larger straight nerve-tubes, and the varicose tube, there are innumerable tubules smaller than either of the others, but nearly uniform in size amongst themselves. These are enveloped amongst the cells and granules, appear on the surface for a short distance, and then sink down again, and are so delicate that they break on applying sufficient compression to expose a single layer of them. On this account their examination is difficult. I have traced some of them into a caudate vesicle, but I could never succeed in making out their connection with either straight or varicose nerve-tubes.

The regular-shaped cells are met with of all sizes ; they are either spherical, ovoid, or pyriform in shape ; most commonly the first. The greatest variety as to shape, size, and arrangement is to be seen in the grey portion of the pituitary body, just where it is joined by the infundibulum. Here the number of nerve-tubes is comparatively small, and the vesicles are therefore less obscured than elsewhere. The nucleus often appears to project at one portion of the parent cell, rendering the outline that of two very unequal circles, like a section of the human eye through the cornea. When two nuclei exist, they generally form two prominences at opposite points of the cell. Some cells, which are yet not caudate cells, present very irregular shapes, as if one end had been pushed out into a pouch-like process more or less elongated. Occasionally an object is presented, which looks as if it had been made by two cells becoming joined together by such a process from one cell having opened into another cell, producing a sort of "dumb-bell" appearance. Here and there may be noticed a pyramid of cells, progressively diminishing from the basement large cell to the small one which forms the apex. On gently moving the glasses, the pyramid of cells is found to move altogether, as if some bond of union existed between them, but this may be accidental, or due to common physical attraction only. The smaller cells are frequently arranged in lines, like a row of beads; or in clusters around the nerve-tubes, like the grains in an ear of Indian corn.

The nuclei vary greatly in size. When sufficiently developed, and viewed in an isolated cell, the projecting nucleus appears to be itself a distinct cell, the outline of one segment of its sphere being visible through the wall of the parent cell, whilst the remainder is external. In some cells there is no projecting nucleus, but one or more round bodies are seen in the interior of the parent cell, which then, except as regards the number of the contained bodies, somewhat resembles the common *volvox globator*. As these internal nuclei are motionless under examination, it is difficult to decide whether they are free or not. But since they present an unbroken spherical contour when viewed through the wall of the parent cell, they probably are free and unadherent. I have never noticed more than three of these secondary cells in one parent cell.

The pigment cells keep together in little clusters when the upper glass is moved upon the lower, in examining the specimen between two glasses; but it is difficult to infer what may be their normal arrangement and relative position to the other parts in a nervous centre. I think, however, that they abound most where the white nerve-tubes separate from each other, as at each end of a spinal ganglion. They are abundant in the *locus niger* of the *crus cerebri*. The pigment spots bear some analogy to the dark granules in vegetable spores and pollen-cells.

The small granules, which are probably minute cells, encrust the tubules, forming a complete bed for them, and filling up the interstices of the other tubes and cells.

The most interesting objects are the irregular cells with projecting processes, appropriately designated by Todd and Bowman "caudate vesicles." In size, shape, and number of processes, they vary greatly. They are large in the spinal chord, and in the ganglion of the vagus, small in the cineritious substance of the brain. The size of the vesicle appears to bear a direct proportion to the number of processes connected with it. The processes seem to be small tubes, resembling the tubules above referred to, which they probably are. Sometimes one or more processes may be distinctly traced from one caudate vesicle to another, being then intervesicular, or merely commissural as regards the vesicles. Another process may be seen to pursue a longer course, and then to become varicose. I have never happened to see more than two processes which thus became varicose connected with one caudate vesicle; this may indicate the truth, or may be owing to the difficulty of following the tubules for any distance, since when a caudate vesicle is isolated, its processes are broken off at various lengths; and when surrounded by other cells, its processes are readily lost to view by dipping amongst the rest. These varicose vesicle-processes do not differ in size, or in any other obvious respect, from the varicose tubes. In ganglia I have clearly distinguished direct continuity between a nerve-tube emerging from the ganglion, and one of these varicose tubules leading to a caudate vesicle. I have seen the same in the spinal chord between the caudate vesicles and the nerve-tubes of the spinal nerves; but I have not succeeded in detecting this connection in the grey matter of the cerebral convolutions.

The assumed discrepancy in the observations of the older microscopists was less owing to actual error than to a spirit of exclusiveness which would not acknowledge the co-existence of a variety of appearances in the same object, certain of which proved most striking to one observer; certain others to another. Thus the thin, translucent, irregular-sized globules of Leuwenhoek; the rows of granules of Della Torre, and of Prochaska; the roundish corpuscles lying amongst intestine-like convolutions of nerve-tubes, described by Fontana; the globules, fibres, and albuminous fluid of Bauer and Home,—are all to be seen, and are doubtless for the most part the same objects seen with various degrees of accuracy.*

In noticing the various appearances presented by the grey cells, it is difficult to avoid the conjecture, that we are viewing nerve-cells in all stages of growth; that the mere granule is the early stage; the large cell with its contained nuclei, or its pouch-like bulging, the mature stage. That nerve-cells are generated in the two ways so universally manifested in cell-life, viz. by budding from the side of a matured cell, and by the rupture of a parent cell setting free the lesser cells previously contained within it. Certain longitudinal loop-like arrangements, met with especially in the ganglia of the fœtus, lead us to admit the conclusion of Schwann, that the tubules originate in the linear apposition of cells, which afterwards open into each other, as in the genesis of the vascular tissue in vegetables.

The grey part of nervous tissue is now admitted to constitute the force generating, the dynamic apparatus. How is the *vis nervosa* transmitted from the grey cells into the white tissue? Those who believed that no direct communication could be made out between nerve-tubes and nerve-cells, but that the sole relationship consisted in the close apposition of the two kinds of nervous tissue, must have assumed that the nervous force passed through the walls of the nerve-tubes, or else along the exterior only of these tubes. But if every nerve-tube terminate by opening into a caudate vesicle, there would in this way be a direct continuity of internal surface between cell and tube, (force-generator and duct.) And impossible though it is to demonstrate that all the white nerve-tubes open into vesicles, it is most probable that they do so. We cannot at least discover any other mode of termination for them in any nervous centre. By one physiologist, the nerve-tubes are stated to end in loops amongst the grey matter; by another, in bulbs; by a third, in division into smaller tubes or fibrils. Such appearances are easily seen, but are in reality, I believe, fallacious.· All nerve-tubes that can be distinctly traced to their natural termination,—and they are very few,—open into caudate vesicles. A nerve-tube is never seen to join another without the existence of a dilatation at the point of junction, constituting an irregular-shaped cell, larger or smaller, according to the number of tubes joining; hence the caudate vesicle is the only medium of communication between the nerve-tubes. Still the great number of regular-shaped grey cells into which no tubes can be traced, their close application and intimate connection with the nerve-tubes, and the translucency, greater delicacy and smaller calibre of the tubes themselves whilst surrounded by the grey vesicles for some distance after their connection with caudate vesicles, render it extremely probable that the regular-shaped cells of all sizes generate nervous force, which passes through the walls into the interior of the nerve-tubes. There is good reason for considering that the white matter of Schwann, (the white coat of the larger nerve-tubes), acts as a non-conductor, obstructing the transfusion of nervous force during its passage along the interior of the nerve-tube. If this be granted, it is a fair assumption that the grey tubes, tubules, and caudate vesicles, as they do not possess this white coat, will allow the force generated by the cells around them to permeate their walls. The varicose tube has no white coating, and is found only amongst the grey cells. May not the flask-like dilatations be a normal arrangement for the purpose of exposing at different points a greater extent of surface to the surrounding grey cells, than the mere ordinary calibre of the nerve-tube would permit? Are not the caudate vesicles and varicose tubes permanent structures; whilst the nucleated cells are temporary, and evolved rapidly during the functional exercise of nervous tissue? The caudate vesicles may perhaps be compared to central termini, where different lines join, and where the chief supply of power is furnished; the varicose dilatations to side stations where additional force may be obtained.

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* See an Analysis of Ehrenberg's Views by Dr Craigie, Ed. Med. and Surg. Rev. vol. xlviii. p. 257.

The larger nerve-tubes have been characterized as semi-opaque and white. It would be more correct to say that they have a great tendency to assume these peculiarities, since when perfectly fresh, they are neither white nor opaque, but become so even whilst under examination,—just as the retina, though perfectly transparent during life, becomes white and opaque after death.

On compressing a portion of any white nerve-cord, the cut ends assume the appearance of a woolly mop. This is owing to the squeezing out from the white nerve-tubes of innumerable small granules, not distinguishable by myself from the gray granules so abundant in the nervous centres. Is the central band or axis of a white nerve-tube made up of these granules linearly disposed? And, is each granule itself a minute nerve-cell? On examining a nerve-cord in a living animal, (in the axillary or inguinal regions, for instance,) or in one just killed, before the neurilemma has lost its transparency, each nerve-cord is seen to consist of the nerve-tubes invested with a finer and inner membrane, forming a longitudinal fasciculus, which is either bent upon itself in a zig-zag manner, or otherwise coiled round in the canal of the denser outer neurilem, with its spirals so close as to present a zig-zag appearance. The light is so much refracted by the glistening surface of the object under a lens, that I could not satisfy myself which was the true arrangement, *i. e.* whether the angles of flexure were sharp or obtuse. When a nerve-cord is about to divide into two or three smaller ones, the requisite number of fasciculi first appear by the nerve-tubes separating into bundles, which are at first intertwined altogether, but by degrees become distinct, lying side by side, and each presenting the zig-zag arrangement. Hence we may see one larger line of zig-zag flexures splitting into three smaller lines of zig-zags, each of which presently obtains a distinct outer neurilem, and is then a distinct nerve. If one of these nerves has its outer sheath scratched through, and its zig-zag nerve-fasciculus drawn out, the latter remains elongated, and looks opaque and fleecy, the zig-zag appearance being lost. Hence the arrangement is not due to elasticity of the inner sheath. As this zig-zag arrangement allows at least three lengths of nerve-tube to be contained in one length of the external neurilem, it must be subservient to some other purpose besides that of obviating injury from sudden stretching of the nerve. Like the convolutions of the cerebrum, the laminæ of the cerebellum, the striæ of the *corpus striatum*, the curl of the *corpus rhomboideum*, or the lobules of a gland, these flexures will permit of a greater extent of surface in a given space, than could be otherwise obtained. The interior surface of each nerve-tube is thus rendered very extensive, whilst the tube itself is most conveniently and safely packed. If we decline to admit that the minute granules contained in the nerve-tubes do of themselves generate nervous force sufficient, speaking metaphorically, to change the nerve for conduction, we must allow that, by the arrangement described, a certain accumulation of nervous force is favoured.

The zig-zag appearance being invisible as soon as the outer sheath has become opaque, is not to be seen in the ordinary dissection of the human body, but it is distinct in the nerves of a limb just amputated. I have found it to be presented alike by spinal and cerebral nerves, with the exception of the olfactory, the optic, and the auditory; by sensational and motor nerves, when not so large as to preclude it by their density; and by the roots of all the spinal nerves. It is met with in the calf, sheep, dog, cat, fox, horse, ass, hare, rabbit, guinea-pig, hog, rat, mouse, and weasel; in the pigeon, owl, rook, duck, goose, and fowl; in the frog, and water-newt; in cod fish, skate, trout, carp, and eel; and may therefore be considered constant in the vertebrata.

Valentin doubts whether the inferior ganglion of the vagus be really ganglionic at all. In the young rabbit, I have found it a very good source for furnishing characteristic specimens of the general structure of a ganglion. Slices suitable for examination between glasses are conveniently obtained from the spinal ganglia in the frog, from the sympathetic near the psoas muscle in any small young animal, and from the ganglion of the vagus in the eel, and in fish generally. In water animals the connecting tissue seems looser, and the nervous elements separate on the slightest pressure. The apparent anatomy of the Gasserian ganglion gives a good idea of the minute arrangement in a spinal one. The white nerve-tubes of the posterior roots of the spinal nerve separate immediately on entering the ganglion, and cross each other at an acute angle. After passing through the substance of the ganglion, they come together again, and, accompanied by additional nerve-filaments from the gray cells of the ganglion itself, emerge in as many fasciculi as there are primary divisions of the compound nerve formed subsequently by conjunction with the anterior root. The nerve-tubes of a given fasciculus come out of the ganglion at different points; so that, dividing the anterior margin of the ganglion into upper, middle, and lower portion, a fasciculus which leaves the lower part derives nerve-tubes from the upper and middle divisions as well, and *vice versa*, producing in this way a crossing of nerve-tubes, intricate in proportion to the number of fasciculi. Some nerve-tubes pass round the sides of the ganglion in a curvilinear manner, but not, as far as I could ascertain, so as to form any regular boundary. When compressed, the larger nerve-tubes keep together in bands, leaving the other parts of the ganglion in the interspaces, and hence the normal arrangement cannot be thus ascertained. Enough, however, may be distinguished to justify the statement that the nerve tubes are closely connected with, and imbedded in, a nidus of cells, granules, and caudate vesicles, or, in other words, of cells of various kinds. A nerve tube accidentally isolated is often seen to have a string of cells applied along its walls.

I have thought that the nerve-tubes appeared rather smaller in calibre, and their walls thinner whilst passing through the ganglion; but I am not satisfied of this, and cannot trust my own attempts at measurement. I have never seen a white nerve-tube terminate in a ganglion, either by opening into a vesicle, or in any other way. If not broken under manipulation, nor lost by its disappearance under other objects, every nerve-tube entering from the spinal cord could be traced through the ganglion without interruption of its continuity, and without becoming varicose. The varicose tubes are less numerous in the ganglia than in the brain and spinal chord, and are never noticed beyond the extent of the grey ganglionic matter. Small non-varicose tubules are seen running in every direction, but to the greatest extent across the course of the larger nerve-tubes. Thus, whilst the latter pursue for the most part a longitudinal course through the ganglion from end to end, the former chiefly pass transversely from side to side. I have never seen these tubules become continuous in the ganglia with the larger nerve-tubes. When traceable at all, they have terminated here as elsewhere in caudate vesicles. Irregular ring-like arrangements of caudate cells and their intervesicular processes, are occasionally observed. Examples are figured from the cœliac ganglion of the kitten, and from the Gasserian, vagus and spinal ganglia, and from the brain in the rabbit.

Some stress has been laid on the obvious difference in external character between the spinal and the sympathetic ganglia.* The spinal ganglion is regular and uniform in shape and size, and constant in its relation to its afferent and efferent nerves. The sympathetic ganglion is irregular and variable in shape and size, and inconstant in its relation to the nerves connected with it. Processes of grey semitransparent nervous tissue are seen emerging from a sympathetic ganglion; in a spinal ganglion no grey fibres leave separate and distinct from the fasciculi of white nerve-tubes. Microscopic examination proves that whilst in a spinal ganglion the white nerve-tubes are arranged with regularity—a given number entering at one extremity, and leaving at the other; in a sympathetic ganglion, the white nerve-tubes enter and leave in any and every direction; some passing straight through several ganglia in succession before they are finally distributed; some leaving every ganglion immediately for their destination. The sympathetic ganglia anatomically differ from the spinal, therefore, in the less regular course of the white nerve-tubes through them, and in the greater predominance of grey cells and tubules. The white nerve-tubes transmitted have no closer connection with the grey cells than in the spinal ganglia; they never terminate in the sympathetic ganglia. The fibres of the sympathetic grey nerves, gelatinous fibres as they have been termed, differ from the white nerve-tubes in the following respects. (1.) They are smaller in calibre. (2.) They have occasionally flattened bodies here and there in their course. (3.) They are translucent, and of a yellowish grey, or pale straw colour. (4.) They do not present a double outline of walls. (5.) They are straight in their course, and are not arranged in a zig-zag manner.

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* Procter on the Sympathetic, p. 29.

It is difficult to decide whether these fibres are tubular or solid, since their small calibre, their delicacy and translucency, prevent the defined double outline of the larger white nerve-tube. It may be that the ganglionic nerve-fibre is not essentially different from the white nerve-tube, excepting in the non-possession of the white matter in its walls. We may conjecture that the white nerve-tube insulates the force it conducts; whereas the grey one does not; that the white allows, from its flexions, of more accumulation than the grey tube. The nerves of animal life must act with suddenness and intensity. The organic processes require a more gentle, gradual, but persistent supply. To perceive a pin's breadth, or the traction of a single hair, a sensational nerve-tube must have its action insulated; and to convey commands to a given muscle, the motory nerve-tube must be capable of acting singly, or almost so. But there appears no such necessity for the purposes of an organic nerve. In health, uncontrolled by sensation, not necessarily affected by voluntary movement, a secreting surface does not require that one point only shall be influenced at one time by its organic nerves. All the nerves from the same ganglion supplied to it may act in concert without inconvenience. If one is required to act, all may act. Hence, not only is insulation of the organic nerve-filaments not required, but the absence of it is possibly a means of ensuring regularity and consentaneousness of action.

Distribution of the ganglionic nerves.—Whenever a ganglion is situated near to an artery, its nerves apply themselves to the coats of the blood-vessel, intertwining around it "like ivy round the oak." Hence, Soëmmering termed the sympathetic, par excellence, the nerve of the arteries. But the general fact of anatomical connection is no proof of this, since the nerves might attach themselves to the arteries merely as furnishing them convenient guides and supports to their common destination. Nor can any direct argument as to function be drawn from the fact, that "the great sympathetic bears a strict relation in its development, to the activity and force of circulation in the four classes of vertebrata, in whom alone a red-blooded circulation exists,"* since such increase of energy in the vascular system is always accompanied by a corresponding augmentation of the sensorial and the other higher endowments. But when we find that wherever any additional vascular apparatus exists, it has invariably a corresponding supply of ganglionic nerves, *e. g.* the free distribution of nerves to every *rete mirabile*, whether in the neck, as in ruminants, in the spinal canal, as in cetaceæ, or in the thorax, as in the porpoise, there appears to be some ground for assuming a functional relationship between the ganglia and the blood-vessels The sympathetic and the ganglionic system in general, throughout the vertebrate series, are not developed in accordance with mere activity of nutrition, but in exact proportion to the degree in which organic activity requires to be varied in response to the wants of the system. This coincidence is so universal that it must point to the truth. And when we find, moreover, that in each individual animal, the situation and connexions of the ganglia correspond to the same anatomical law, we have particular instances to confirm the general observation. The secretion of hair requires greater nutritive action than the ordinary performance of the other functions of the integument; yet the scalp has no extraordinary supply of ganglionic nerves. The formation of scales must demand considerable organic activity in the outer covering of the fish, yet so small are the spinal ganglia in osseous fishes, that their very existence has been questioned. They are more developed in the sharks and rays, which must be considered as decidedly the highest of the fishes. In craftiness, courage and determination in the pursuit of prey, they clearly evince superiority of mental endowment. They have the integument covered here and there with irregular rough scales, or with pointed tubercles, but are not, like osseous fishes, completely enclosed in a scaly coat of mail. Hence, the surface is more impressible, and the dermoid mucous secretion more under the influence of external impressions. In the crocodile, tortoise, and snake, covered in as they are with scales, horny shields or bony plates, the spinal ganglia are small in comparison with what we meet with in the naked amphibia. In the toad, frog, and water-newt, the delicate, sensitive, and vascular skin is largely supplied with glandular follicles. The spinal ganglia in these reptiles are very large. The acrid nature of the secretion on a toad when alarmed or irritated, may exemplify altered organic activity produced through the medium of the spinal ganglia. The transition stage of Batrachian reptiles is the analogue in vertebrata of insect metamorphosis in invertebrata, and furnishes equally interesting additions to our knowledge of the nervous system. In confirmation of the above, we have the spinal ganglia proportionably more highly developed in the reptile stage (full grown frog), than in the fish-condition of the same animal (tadpole). Ganglionic plexuses accompanying the visceral arteries in fishes are most distinct on the stomach, generative organs, and swimming-bladder. The functional activity of the two first is manifestly subject to frequent variation. That of the swimming-bladder is probably equally so. Whether it be subservient or not to the aëration of the blood, this viscus is unquestionably a floating or sinkimg organ under the control of the animal, which by muscular compression can at will increase the density, or, where the existence of a valvular opening into the gullet permits it, cause the expulsion of the contained gases. The refilling of the bladder, or the rarefaction of its contents in consequence of diminished pressure, by which the fish is enabled at pleasure to lessen its specific gravity, and thus rise to the surface at a small expenditure of muscular power, necessitates a consciousness on the part of the animal of the existing state of the swimming-bladder. Hence nerves of sensation from the vagus are furnished. A beautiful network of arteries and veins ramifies between the coats either over the whole of the bladder, or otherwise, is collected in the form of a vascular mass at one spot. In either case, ganglionic nerves proceed to this vascular apparatus. Acting, as it were, under the dictation of sensation, or, if not that, at least of spinal perception, the nerve-tubes supplied by the vagus pass through the ganglia which furnish the ganglionic filaments. In the same way ciliary twigs from the third nerve pass through the minute ganglion, which constitutes the bell-shaped nodule known as the *Campanula Halleri*, on the vascular falciform process in the eye of the sturgeon, and of some osseous fishes. Birds cannot have any great amount of cutaneous sensibility, yet their spinal ganglia are large in proportion to the size of the body. When we consider, however, the periodical variations in the state of their plumage, the half-moulting in spring, the entire moulting in autumn, and the extent to which experience shows the appearance of the bird is affected by its general health, there is sufficient reason for concluding that the highly organised feather-bulbs are materially influenced by the state of the internal organs. Besides this, several portions of the surface where much friction takes place have no feathers, but are soft, moist and covered with down. Here the integuments may differ but little from those of man in their functional endowments. The complexity of the structural arrangements for the formation of feathers should not of itself, if the principle laid down be correct, require any additional development of the intervertebral ganglia, and cannot therefore be an explanation of it. There is no stronger anatomical argument against the view that nerves are essential to extensive nutritive actions, than the comparative paucity of ganglionic nerves furnished to the organs of generation in the female of all vertebrate animals. These organs are endowed with ganglia sufficient to subject their nutritive processes to the influence of the rest of the economy, but in no degree adequate to superintend the minute atomic changes going on, for instance, in the myriads of ova contained in the ovary of a cod-fish or a herring. If this be true, what purpose could be subserved by the ganglia of the gravid uterus enlarging during gestation in mammalia? They have not to furnish the power, the *vis formativa*, required, but merely to be the media through which vascular activity may be modified. For this, their normal size and relation to the blood-vessels would, theoretically, appear sufficient. I have dissected an ape, whose uterus contained two full grown fœtuses, without being able to perceive any increase of size in the ganglia supplying the parts. The spermatic nerves of the common sparrow undergo no apparent change during the excessive alteration in the magnitude of the testes, which takes place in the spring. In January the testes are the size of pin-heads; in April they are as large as small marbles (Hunter).

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* Langston Parker.

In the common fowl, during the period of laying, the transparent membranous connections of the left generative organ permit the nerves to be easily made out immediately after death. The number of ova of all sizes from the grain-like ovulum to the perfect egg, the great vascularity of the outer membrane of the larger ovisacs, the injected vessels in the ruptured calyces which are undergoing absorption, the secretion of albumen by one portion of the oviduct, the deposition of shell by another, and the constant production of lubricating mucus by the whole, sufficiently evince how extensive must be the organic processes constantly proceeding. Yet the ganglionic supply is not large: nor to common observation is it at all greater than when the ovary is inactive. White nerve-tubes are sent to every part, but most freely to the oviduct. A small ganglionic mass connected with the semi-lunar ganglia furnishes the grey nerves, which radiate towards every portion of the oviduct, but are chiefly distributed to the grape-like ovarium. The numerous white nerve-tubes are also derived from fasciculi which had previously passed through the semi-lunar ganglia. They all pass through the ovarian ganglion before diverging, like the intestinal nerves in the mesentery, in their course between the layers of the musculo-membranous mesometry which ties down the oviduct. The lowest portion of the duct, viz. that between the calcifying dilatation and the cloaca, has in addition white nerve-tubes from the lumbar nerves which do not pass through any ganglion. The most important part of the generative organ, the ovary, in which the first step towards the development of the future animal takes place, is more influenced than the rest by other organs. It has therefore a larger proportion of ganglionic nerves, through which its organic actions may be affected. Having no muscular power, it requires no motor nerves; but in order to communicate its own state to the rest of the system, percipient nerves are necessary; hence it possesses some white nerve-tubes. As any excitement of the ovary requires increased nutritive activity of the part, the white nerves pass through the ovarian ganglion in order to call into co-operation the organic nerves on every occasion. The involuntary movements of the oviducts require a large supply of motor nerves. There are, therefore, white nerve-tubes in greater quantity; and as in proportion to the necessity for active muscular movement will be the necessity for liberal secretion of the required products, so also do the motor white nerves pass through the source of the ganglionic nerves of the oviducts. But as the final expulsion of the egg when perfected may be conveniently left to the will of the animal, the lowest portion of the expelling tube is furnished with non-gangliated nerves, which we may assume to be nerves of voluntary motion. It is obvious that if the ganglia supplied nerves for involuntary muscular motion, the arrangement here would be misplaced. More grey nerves should have proceeded to the muscular parts, and fewer to the non-muscular, which is the reverse of the fact.

From the general distribution of the ganglia we may justly infer that some very intimate functional connection subsists between these nervous centres and the blood-vessels, yet to define the precise nature of the relation we require to know the exact termination of each portion of the compound ganglionic nerves. Our information on this point is at present very imperfect, but as far as it goes, it supports the supposition that purely ganglionic nerves are never destined for muscular fibres, but terminate exclusively in capillaries or secreting structures. By the use of diluted acetic acid, Purkinjé has succeeded in rendering the pia mater of the spinal chord so transparent, that its nerves become visible to the naked eye. "A most beautiful network of the finest nerves" presents itself, the filaments of which belong to the vegetative system. Their discoverer has not been able to find any connection between these filaments and the roots of the cerebral or spinal nerves, though if any such existed, it would be easily discovered, from the greater size of the nerve-fibres of the cerebro-spinal system. " A part of these nerves are connected with the delicate coverings of the spinal chord by the anterior, and others by the lateral spinal arteries which pass in pairs through the spinal (intervertebral) foramina. In both cases they appear to arise from the sympathetic system." In the cranial dura mater, " the largest collection of these nerves are to be found close to the anterior, middle, and posterior arteries where they enter this membrane, and it is not very difficult to trace them running with the arteries back to their origin in the sympathetic system. In the dura mater of the spine I have not found a trace of nerves." The Professor, however, proceeds to explain that this remark applies only to the dura mater proper, which separately surrounds the chord ; whereas the outer of the two layers into which the cranial dura mater splits at the *foramen magnum*, and which is applied to the walls of the vertebral canal as a periosteum, is largely supplied with fine nerve-filaments, which appear to communicate with the sympathetic system through the intervertebral foramina. The cornea, the periosteum of the tibia, and several other non-muscular tissues were found to be furnished with similar nerves.[*] Purkinjé has not at present detected them in serous membranes; but Bourgery states, that " he has observed in great abundance the nerves of serous and synovial membranes ;"[†] and Mr Rainey believes that he has detected not only filaments, but ganglion-globules also, in the arachnoid. He considers, moreover, the supposed epithelial cells situated on the choroid plexuses to be in reality ganglionic vesicles. The choroid plexuses are, unless Mr Rainey's observation be correct, a remarkable instance of *retia mirabilia* unsupplied with nerves. I have failed in my attempts to distinguish either nerve-corpuscles or filaments in the choroid plexuses, and Purkinjé states that no trace of nerves can be found in them ; notwithstanding, therefore, the superiority of positive over negative evidence, to my own mind the question is undecided.

Whether the ganglionic nerves end by forming loops, in free extremities, or in bulbous enlargements, is entirely unknown. But whatever may be the precise mode of their peripheral termination, it is quite certain that, in common with all other nerves, they must exercise an influence beyond the absolute extent of their nervous filaments. To doubt this is as unreasonable as it would be to question whether the organizable constituents of the blood extend beyond the limits of the vessels which convey them. As the nerves have no other object but that of affecting non-nervous textures, the nervous force must of necessity be imparted to other tissues than the nerve-tubes which conduct it. And if communicable to one living molecule, it may be to many. Hence,

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[*] Dr Gull's Translation, Medical Gazette, 1845.

[†] Ranking's Abstract, vol. xi.

Reil's idea of a "nervous atmosphere" is far from absurd. Bichat objects that it is vague and indefinite. It is so as regards our knowledge of its extent of operation, and of its possible variation. But there is little to cavil at in the expression, and nothing vague in the thought. Without emanation of influence of some kind, the ascertained power of the nervous system would be inexplicable. How could a motor nerve affect muscular fibre unless its energy, whatever its nature, proceeded out of the nerve to the muscle? How could an organic nerve affect a cell unless its force were communicated by the nerve to that cell? The nature of the force is unknown. So is the nature of light and heat. But as matter cannot be illuminated or heated without the communication of these, so it is impossible for a gland or a muscle or a secreting membrane to be influenced in the least by the nerves, unless the nerves can send out from themselves the force they convey. It would be interesting to find that all nerves, cranial, spinal, and ganglionic, are alike divested of their insulating walls at their ultimate terminations. The incomplete information at present possessed certainly favours the supposition. Hannover and Kolliker state that the peripheral filaments of the encephalic and of the spinal nerves become so much finer as to be with difficulty distinguished from grey ganglionic nerve-fibres. In the Pacinian corpuscles, the entrant nerve loses its double contour and white wall the instant it reaches the inner capsule, and is thenceforth a grey fibre. In the cutaneous and lingual papillæ the nerves are seen to become grey ere they are lost to view. In the superficial follicular bodies discovered by Savi in the electric torpedo, the sensational nerve-twig is stated to enlarge within the follicle, to supply filaments to the contained granular matter, "which nearly resembles the amorphous grey matter of the cerebral hemispheres," and to emerge much reduced in size; but whether or not it is divested of the white matter of Schwann is not remarked.*

The final loops of muscular nerves are not stated to be deprived of their insulating walls; but from Mr Bowman's researches we glean the important fact, that mere areolar tissue can offer no obstacle to the passage of nervous influence, since the sarcolemma in every instance separates the muscular from the nervous fibre. Unless the *vis nervosa* can pass from the generating nerve-cell into the nerve-tube, we can understand nothing of the origin of nervous force; and unless it can pass out of the peripheral portions of the nerve-tubes, the undoubted effects produced through the medium of the nerves are unaccountable. We can demonstrate neither; but we must admit both. If, in addition, there be no reason to doubt that the *vis nervosa* is one and the same kind of force by whatever centre generated, it is fair to assume

* Art. Pacinian Bodies: Cycl. of Anatomy and Physiology.

that all delicate tissues in immediate contiguity to grey nerve-vesicles will be more or less under the direct influence of those vesicles. Hence the non-necessity for ganglionic nerves in the *plexus choroides*. It is no refutation to assert that this is mere conjecture. It is not open to positive proof, and should be estimated solely by the reasonableness or groundlessness of the argument.* It would also result, that although the ganglionic nerves be especially the organic nerves, yet their presence is unnecessary for the purpose of regulating nutritive activity where any other kind of nerve exists divested of its insulating white matter; provided no very intimate consent of action with other tissues supplied with ganglionic nerves is required. The molecular changes of nutrition in muscle may therefore be modified directly through the medium of the muscular nerve; the nutrition of sentient papillæ in the tongue and skin, and of the tissues of the Pacinian bodies, through the sensational nerves. The nutrition of these parts needs modification in accordance with the discharge of their respective functions only. It is quickened in activity when the muscular nerve excites contraction, or when the sensational nerve fits the papilla for receiving impressions. It is not susceptible of frequent alteration independently of these nervous actions, as the secretions of mucous surfaces are, *e. g.* the tongue. If it were, according to theory, ganglionic nerves should exist.

Of the exact mode in which the blood-vessels are supplied with nerves, whether their nerves are cerebral, spinal, or mixed, how they terminate, and where, we are quite ignorant. In the extremities, twigs from the cerebro-spinal nerves which accompany the arteries may be traced into the coats of the vessels, but their actual termination is not known. An anatomical connection between the nerves and the smaller vessels "has been proved by Purkinjé, Valentin, Remak, Henle, and others, who have seen the finest nervous filaments on the wall of the cerebral and other blood-vessels, of less than one-fiftieth of an inch in diameter."†

We find but few simple unmixed ganglia for our notice, but wherever these do exist, their nerves invariably supply either secreting membrane, or vascular tissue; never muscular fibre. Remak has observed purely grey ganglia on the surface of the heart, in the posterior pulmonic plexus, and on the walls of the urinary bladder. Müller and more recently Purkinjé describe similar ganglia in the *corpora cavernosa penis*, where they cannot supply any thing but vascular tissue. Connected with the descending grey fibres from the spheno-palatine ganglion in the sheep, calf, and probably in other herbivora, we may frequently meet with minute, oval, round or fusiform enlargements; grey, translucent, and unmixed with white nerve-tubes. The aggregate of the efferent fibres from one of these simple ganglia exceeds in quantity the gray nerve on which the nodule is placed. The purely gray nerve-filaments are distributed to the well-developed follicles on the soft palate.

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* See a note in the Lancet for 1846.

† Paget's Report. British and Foreign Medical Review, No. xxvii. p. 288.

In the few instances in which supernumerary ganglia have been discovered in unusual situations, they have never been found connected with the filaments of a motor nerve. Mr Swan has occasionally detected a ganglionic enlargement on the branch of the spiral nerve which lies beneath the extensor tendons of the wrist,—a cutaneous nerve. In two subjects,* Mr Pilcher met with "a gangliform enlargement on the internal nasal nerve, where it is lodged on the œthmoidal bone," a sensational nerve.

If the ganglia do exert an especial influence on the organic processes, inasmuch as these are in action whilst sensation and muscular motion are yet scarcely exercised, we should expect to find the ganglia fully developed earlier than either encephalon or spinal chord. Arnemann and Beclard assert that the intervertebral ganglia are the earliest of all the nervous centres in their appearance, at least in birds. This is probably erroneous, but there can be no question that the ganglia generally are proportionably larger than the other centres, in the fœtus. And if essentially independent of both brain and chord, the ganglia will occasionally be present, where, from arrest of formation, both the other great nervous centres are absent. Accordingly, in anencephalous and arachidian fœtuses, we find the ganglia where there is neither brain nor chord.

The anatomical grounds for deciding, first, that the ganglia superintend and regulate the activity of the organic processes; and, secondly, that they do not in any muscle excite contraction; may be summed up as follows. (1.) The ganglionic nerves possess special centres of origin distinct from those which are known to preside over the other functions of the nervous system. (2.) Nerves from other centres (with the exception of a few communicating gray filaments which connect the gray cells of one centre with those of another in some instances) do not terminate in the ganglia, but merely pass through them, and afterwards supply compound tissues in common with the purely ganglionic nerves. This would have been needless if the ganglia themselves were so related to the tissues supplied, that they could ensure perception, sensation, and muscular action. (3.) In their course, the ganglionic nerves are intimately related to the arteries, the carriers of the matériel on which the organic nerves must work. (4.) By their termination, so far as this is known, they are fitted for influencing capillary actions, and the atomic changes in non-nervous and non-muscular tissues. (5.) Developmental and abnormal anatomy support these conclusions.

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* Grainger in Cycl. of Anatomy, Art. Ganglion.

On the other hand, certain recent experiments of Volkmann are considered to have proved that the heart, and the muscular viscera of the abdomen, contain within themselves nerve-centres of muscular motion. Such centres, if they exist, can be no other than the ganglia.

Experimental evidence concerning the alleged motor-power of the ganglia.—By employing the continuous current of an electro-magnetic apparatus, Volkmann believes that he has established the following laws:—

(A.) When an excitor nerve only is electrified, any muscular movement which results is intermitting and orderly, the muscle contracting and relaxing alternately. This kind of muscular contraction may be distinguished as alternating contraction.

(B.) When a muscular nerve is electrified either at its origin, or at any part of its course from centre to muscle, the resulting contraction lasts during the continuance, but ceases on the cessation of the stimulus. Volkmann terms this form continued contraction.

(C.) But when the stimulus is applied to the centre in which the motor nerve arises, the muscular contraction not only continues during the application of the electric force, but persists without remission for some time afterwards. Persistent contraction.

It is very difficult to elicit excito-motory action in any other way than by stimulating the peripheral terminations of the excitor nerves. Todd and Bowman illustrate this by the great excitability of the skin of the thumb of the male frog. On shaving off the papillæ at this part, the susceptibility is lost, and irritation, which before would have caused the animal instantly "to assume the attitude of grasping," is now of no avail. The same fact may be exemplified in another way.

Experiment XXV.*—A large frog was pithed in order to remove the influence of sensation. The skin was stripped for an inch off the right side of the back, so as to expose three cutaneous spinal nerves passing transversely towards the abdominal integuments. A longitudinal incision through the skin in front of the abdomen prevented any interference from the cutaneous nerves of the left side.

134. Irritating with a needle, and twitching the exposed nerve-trunks produced no effect.

135. Tickling and pinching the skin adherent to the abdomen on the right side of the longitudinal incision, and opposite to the reflected flap of thin integument, caused motion of all the extre-

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* Whilst these pages were passing through the press, I found that this experiment was almost identical with one of Dr Marshall Hall's. I was not aware of this at the time.

mities. After the lapse of eighty seconds no effect could be produced. Hence irritation of a coarse mechanical kind applied to the nerve-terminations fitly organised to receive such impressions, caused excito-motory action until the nerve-trunks had, by exposure probably, become incapable of conduction; but similar irritation of the nerve-trunks, though applied first, had no such effect.

Where is there a purely excitor nerve which is unconnected from its origin to its termination with motor fibres? The physiological evidence of the existence of excitor fibres is founded on the results of stimulation of surfaces, which could not directly excite motor fibres. But a continuous current of electricity passed along a cutaneous nerve, before it arrived at the central origin, would come in close contact with motor fibres. What proof have we that it affects the nerve-centre at all? It may react along the muscular nerves, to which the excitor fibres of necessity first convey it. The posterior roots of the spinal nerves alone would offer an exception. In applying a continuous current to them, it would be difficult to restrict its influence merely to the short portion of nerve-root. But assuming that the electric current does act through the medium of the nerve-centre, how can we prove that it does so by calling forth the normal action of the dynamic grey cells, and of the motor nerve-tubes to which they give origin? And unless this be established, any result is worthless as regards the function of the nervous centre. It is stated that if the magneto-electric stimulus is applied to the brain or spinal chord, persistent contraction of the voluntary muscles results. For how long a period after withdrawal of the stimulus is not mentioned. I have made many experiments with a rotating magnet, of the power usually employed for hospital patients. So long as the electric influence was carefully restricted in its passage to the grey portions of the encephalon, no muscular motion in any instance resulted. When applied to white matter any movement resulting was alternating or continued, according to the amount of electric force, and the care with which the instrument was kept applied, and the state of muscular irritability of the animal; never persistent. But supposing the fact to be as stated, that a continuous electric current applied to the brain or chord will occasion contraction of some voluntary muscles which lasts for a certain time after ceasing to apply the electricity, this is no proof that the centre itself is acting in a similar manner to its normal mode in the motor nerves. It may be that a greater number of motor fibres destined to a given muscle have been subjected separately to the stimulus, than can be the case where an excitor or a muscular nerve, or, as must usually happen, where a compound nerve alone is stimulated, and thus that the amount of stimulus is greater.

The heart, stomach, and intestines do not present continuous contraction in answer to stimulation of the brain and chord. Volkmann hence infers, that these organs derive no motor fibres from either brain or chord. How much this is opposed to anatomy need scarcely be noticed. Direct irritation of the motor nerve of a volunary muscle causes alternating contraction only, unless the irritation be applied without intermission by means of the electro-magnet. And even with this instrument, when the power is feeble, the muscular contraction, though irregular in its duration, is not uniformly continued as Volkmann asserts. The continuousness of the muscular contraction, therefore, depends not upon any property of the motor-nerves, but, *cæteris paribus*, upon the degree and continuousness of the stimulation. At the utmost, then, if well-founded, Volkmann's laws could be useful merely as tests for different nerves, whether excitor or motor, but would add nothing to our knowledge of the normal influence of the nervous centres. They would prove that excitor nerves pass into centres, and motor nerves pass out, and that the entrant nerves of the viscera pass into the visceral ganglia; none of which stands in need of such support. They would furnish new facts as to the behaviour of these several kinds of nerves under the physical stimulus of electricity, but no evidence as to their comportment under the natural stimulus of nervous force. Until excitation of the dynamic nervous tissue by the electric stimulus after death has been established, we have no foundation for assuming that there exists the slightest analogy in the mode in which the nerve-tubes are affected by the two forces. The nervous force travels along the interior of the nerve-tubes, and its transmission is at once stopped by placing a ligature around the nerve. The electric force travels along the outer surface of the nerve-chord, and receives no obstruction from such a ligature. The circumstance of the involuntary muscles, therefore, presenting alternating contraction only, in answer to electrifying the brain and chord, is, if correct as to fact, no proof that the ganglia are motor centres to the viscera; it is a proof merely that the ganglia have a similar relation to the passage of the electric force along the nerves connected with them, to that of the brain and chord in reference to the nerves connected with them. But even this is questionable. The peculiar characteristics of the muscles must be borne in mind. The muscles supplied with nerves connected with ganglia, and the muscles supplied with cerebro-spinal nerves, are themselves different in both structure and endowment. Mr Bowman's admirable researches have proved that striped muscular fibre, when isolated from every vestige of nerve and blood-vessel, will itself contract under irritation; that unstriped fibre will also contract on stimulation, but not precisely after the same manner, although, as far as is known, there is no difference in the kind of molecular change essential to the act of contraction. "Wherever the striated structure occurs, we witness an aptitude for quick, energetic, and rapidly repeated movements; while, where it is deficient they are sluggish, progressive, and more sustained."* The electric force applied to nerves has a two-fold action according to the mode in which it is used. When strictly confined in its course to a small portion of nerve isolated from all the surrounding tissues, it can affect the part to which that nerve is distributed only by stimulating the nerve-tubes to give out their nervous force. But unless great care be taken, the electric force is itself conducted to the distant part, where it acts as a direct stimulus. If we can prove that all the effects on muscles obtainable by the operation of electricity in either of these ways, may likewise be produced by mechanical irritation of the muscles themselves, under circumstances which preclude the possibility of the intervention of any nervous centres, it is manifest that the results arrived at by Volkmann are due entirely to the degree and extent of action of the stimulus, the mode of its application, and to the difference of endowment in the muscles experimented on. Pathology furnishes numerous instances of every variety of muscular contraction suddenly induced by irritation of excitor nerves; from the momentary twitches of the face, (alternating contraction,) to rigid tonic spasm, (continued and persistent contraction.) If the *vis nervosa* and the electric force acted alike on the nervous centre, we ought never to see more than the alternating kind. As we can induce all the three kinds of contraction in a living muscle separated from its nervous centre, the influence of the nervous centre cannot be essential to the production of any of them; and consequently the existence of any one is no proof of either the exercise or the non-exercise of the normal force of a given nervous centre. In this case, Volkmann's inference that the hollow viscera derive their nervous stimulus to contraction from the ganglia exclusively, is unsupported by his evidence. In observations which follow, will be found facts to show that the behaviour of different kinds of muscle *per se* must be taken into consideration. But there are some of Volkmann's inferences in which the relation to his own (assumed) laws is not very clear.

After establishing a law on the logical error that because all reflected movements are alternating, all alternating orderly movements must be reflex, it is inferred that the ganglia are the motor centres to the heart, stomach, and intestines, and that the vagus, spinal chord, and sympathetic furnish excitor fibres to the ganglia, because electro-magnetic stimulation of these occasions "orderly movements of alternate contraction and relaxation," (such movements, however, bearing no resemblance to the alternating contraction of voluntary muscles.) Then, as regards the iris, because stimulation of the third nerve causes contraction of the pupil, which is sometimes prolonged after withdrawal of the magnetic stimulus, it is deduced that the ophthalmic ganglion is the motor centre to the iris, and that the trunk of the third nerve conveys excitor fibres to the ganglion. Thus the cardiac ganglion is proved to be a motor centre receiving excitor nerves, because persistent contraction cannot be induced; whilst the ophthalmic ganglion is proved to be a motor centre receiving excitor nerves, because persistent contraction can be induced! And as to the excitor fibres, the spinal excitors are those between surface and centre; Volkmann's centripetal fibres are those between centre and centre, chord and ganglion. Hence there is no parallelism between them. Again, it is inferred that "the motor nerves of the anterior, but not those of the posterior extremities (of the frog), arise from the uppermost part of the spinal chord. For if the anterior part of the chord be electrified, the contraction of the anterior extremities is often prolonged after the withdrawal of the stimulus; but that of the posterior extremities ceases with the cessation of the stimulus. Now, if the motor nerves of both extremities arose in the brain, this difference would be unaccountable; and as, according to the preceding rule, it is stimulus of the central organs alone which induces prolonged contractions, it follows that the upper part of the chord is a centre to the brachial, but not to the sciatic plexus (of the frog)."* There is an important omission here. Volkmann found that persistent contraction of the lower extremities of the frog occasionally did result from applying electricity to the upper portion of the chord. As one instance of success should annul all cases of failure, this exceptional occurrence must not be slurred over. In reality, according to the principles laid down, it would prove the reverse of the conclusion drawn. Volkmann wishes it not to be considered as militating at all against his views, and accounts for the fact by assuming that the electricity was too powerful, and produced persistent contraction of the muscles of the posterior extremities by reflex action! In that case, what proof that the brachial nerves were not excited in a similar way; and what becomes of the law that all movements excited by electro-magnetism in the reflex manner must be of the alternating, never of the persistent kind? A natural law must have no exceptions. I believe it may be objected to Volkmann's laws, that the facts on which they are founded are not sufficiently constant and uniform to sanction their generalization into laws at all. But waving this as being mere matter of opinion, there are several reasons why we must reject the inferences, even if we admit the correctness of the laws. (1.) As the muscles which are supplied with gangliated nerves cannot be made in any way to present alternating contraction resembling that of the voluntary muscles, the non-presentation of it under the stimulus of electricity proves nothing *pro* or *con* with respect to the assumed motor influence of the ganglia. (2.) As every possible variety of contraction presented by voluntary muscles may be elicited without the intervention of nervous centres at all, the production or the non-production of a given kind of contraction through stimulation of the nerves, proves nothing with respect to the manner in which motor influence is normally exercised by the chord or brain over the voluntary muscles. (3.) As the gangliated muscles can be made to present both continued and persistent contraction by stimulating sufficiently their spinal nerves, spinad to the ganglia, if it were true that the occurrence of these kinds of muscular contraction is a decisive proof of the motor nerves having already departed from the centre which supplies them with motor energy, we should arrive at the reverse of Volkmann's conclusion, and infer that the ganglia are not motor centres. (4.) As all the phenomena elicited, together with their variations, can be reasonably explained by referring them to the special properties of muscular fibre, under different degrees of stimulation, and as they cannot be satisfactorily accounted for by reference to any known properties of the nervous system, the former is probably the correct explanation.

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* Todd and Bowman's Phys. Anatomy, vol. i. p. 185.

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* Paget's Report, op. cit. No. xliii. p. 279.

The following experiment will exemplify several of the statements advanced.

Experiment XXVI.—A large kitten was bled to unconsciousness, that appearing to be the best mode of producing insensibility without occasioning great interference with the nervous and muscular systems. A sharp small blade was pushed behind the cervical vessels on the right side, so as to divide the carotid artery, jugular vein, and vagus and sympathetic nerves. As the blood flowed freely, the animal became faint and still, but its heart continued to beat rapidly and feebly.

136. In five minutes the animal appeared to be unconscious. The abdominal viscera were exposed. The stomach was distended with food; the intestines motionless. It is supposed by some that the vermicular movement of the small intestines is constantly or almost constantly going on. This appears to be an error. The muscular coats act so as to produce motion only when stimulated by the presence of contents or by the nerves.

137. The diaphragm was at rest, except that at each stroke of the heart against its upper surface, the muscular fibres nearest to the part struck became firmly contracted for the instant. This contraction extended through the entire length of the fasciculi af-

fected, but did not spread to the rest of the diaphragm. Here was continued contraction of muscular fibre produced by the mechanical irritation of the heart-stroke, and not spreading to neighbouring fibres, because the diaphragm is made up of the striated muscle, and has its fasciculi separated from each other by septa of areolar tissue.

138. The elongating effect of the arterial wave on the mesenteric arteries was very apparent; but it was not perceived that the calibre of the vessels underwent the slightest change.

139. On touching any part of the small intestine, contraction of the part touched occurred instantly, increased for some time after removing the contact, spread along the intestine in both directions, upwards as well as downwards, and finally left the part touched in a medium state of contraction, its calibre being greater than during its active contraction, but less than before it was caused to contract at all.

140. In no case would this local irritation, if applied so gently as not to affect other portions of the tube mechanically, cause movement of a distant portion of the intestinal canal immediately; but it sometimes did this—after the primary contraction by spreading from fibre to fibre had implicated so much of the intestine that its coiling movement could not do otherwise than drag the mesentery and its nerves.

141. The contraction resulting from a gentle touch, together with that which spread from the point touched, was evanescent. But not so if a scratch with a sharp instrument or a pinch with the forceps were inflicted on the bowel in place of merely touching it. The contraction at the point irritated was considerable in its degree and in its length of duration in exact proportion to the amount of irritation inflicted. Thus, whilst mere touching of the peritoneal coat of the intestine produced a dimple which remained for 50 or 60 seconds only, a rough scratch along the length of the bowel caused instant and strong contraction; the irritated fibres becoming firm, prominent, and standing out from the surface of the intestine; and, by contracting much more suddenly and forcibly than the neighbouring fibres, causing a longitudinal crescentic depression, which remained for from five to ten minutes. In similar manner a scratch across the tube of the intestine produced a ring-like constriction which lasted equally long. Gentle vermicular action spread along the bowel from the point irritated, as in the previous observation (§ 139), where the irritation was slighter; and this spreading contraction subsided equally soon, leaving one portion of the intestine as it implicated another. Here we see continued and persistent contraction of unstriped muscle produced by direct irritation of the fibres themselves. The nerves had probably no share in producing the phenomenon; cer-

tainly there was no direct excitement of any nervous centre; hence the occurrence of this form of muscular action is no proof that the nervous centre in which the motor nerves of the part under examination arise, is acting strongly, or indeed acting at all.

142. Scratching the cœliac ganglion had no effect on the distended stomach, but produced vermicular movement of various portions of both large and small intestines simultaneously.

143. Pinching the nerve-trunk accompanying a principal mesenteric artery caused immediate vermicular action of the portion of intestine to which the peripheral filaments of the irritated nerve were distributed; but no further movement of the rest of the intestine than what proceeded from an extension of the primary muscular contraction. Hence, there was no excito-motory action induced.

144. Scratching the surface of the stomach produced contraction at the irritated part, but no general muscular action of the stomach. It never occasioned motion in any distant part. The over-distension of the viscus, however, would lessen the irritability of its muscular fibres, and probably impair also the excitability of the surface.

145. The gall-bladder and the biliary ducts were quite full. Scratching the first caused a slight rugous dimpling of its coats, but no such prominent appearance of the fibres in action as the stomach and intestines presented. No manifest effect resulted from irritating the hepatic and the gall-ducts.

146. The spleen was moderate in size, and resembled a flattened, elongated raspberry. Irritation of its surface had no effect. On cutting it half through, no blood flowed; on dividing its vessels in the hilus, blood flowed freely.

147. The kitten had appeared quite insensible and motionless all this time; it inspired at intervals, and its heart beat rapidly and feebly. The abdominal aorta was now divided; a sudden gush of blood instantly stopped the action of the heart; a single deep inspiration was drawn; the pupils, previously small but sluggishly responsive to the light of the sun, suddenly enlarged to their utmost, and the animal was dead. Thirty-six minutes had elapsed since the division of the cervical vessels.

148. The front of the chest was removed without injuring the pericardium, the transparency of which permitted the action of the heart to be distinctly seen. The right auricle and ventricle were contracting vigorously and in regular order. The left ventricle appeared small, firm, and motionless, being moved at each systole of the right ventricle, but having no independent motion of its own. The left auricle contracted feebly but distinctly, synchronously with each systole of the right auricle. The action was

rather a sudden quivering of the appendix of the left auricle than general contraction of the auricle. The auricles always moved together; were equally motionless when the ventricle contracted; and usually contracted twice for one systole of the right ventricle.

149. The diaphragm was convex towards the thorax. On gently pinching the left phrenic nerve, the left moiety of the muscle contracted; the right portion remaining quiescent. The heart's action, which had become more tardy, was accelerated. After these effects had subsided, on strongly pinching the phrenic nerve lower down than the seat of its former irritation, the left half of the diaphragm contracted suddenly and firmly, and *its muscular portion remained flattened and rigid for eight minutes.* Division of the phrenic nerve did not interfere with this state of persistent contraction, which was thus proved not to be dependent on any given influence of a nervous centre. On mechanically irritating the central portion of the divided phrenic nerve, no movement of the heart whatever resulted. The acceleration of the heart's action noticed on irritating the nerve before its division was not due, therefore, to any excito-motory influence through the medium of the phrenic nerve, as in similar experiments some have erroneously supposed, but probably to the effect of the contraction of the diaphragm in disturbing the position of the heart.

150. The right ventricle had not moved for two minutes; on pinching the left vagus at the root of the neck, the right ventricle contracted four times and then ceased. In performing this, care was taken not to move the heart or pericardium by any traction of the nerve.

151. The pericardium was now opened, and the surface of the heart exposed. The auricular appendices twitched slightly, but the ventricles were motionless. The contact of the air appeared to have no effect; but the irritability might have been becoming exhausted. Scratching the left ventricle produced no general action in it, as it had remained contracted from the first (*persistent* contraction without direct irritation either of nervous centre, or nerve; due possibly to the sudden removal of the contents of the left ventricle when the aorta was divided having occasioned an extreme contraction of the muscular fibres); but there was slight elevation of the muscular fibres at the part irritated. When the left ventricle was so pressed as to communicate movement to the rest of the heart, the right ventricle and both auricles contracted slightly; but irritation of the left ventricle without pressure had no effect on the rest of the organ. Pinching the right ventricle caused its contraction, followed by that of both auricles. Pinching the right coronary vessels and nerves in the right auriculo-ventricular groove, caused contraction first of the ventricle, and afterwards of both auricles.

152. All the vessels were cut through close to the base of the heart, and the organ removed. Slight irritation of the right ventricle caused slight contraction of the fibres irritated, but nothing more. Stronger irritation of the ventricle induced more extensive action of the right ventricle followed immediately by contraction of both auricles. After the lapse of five minutes from the time of removing the heart, irritation failed to induce any muscular contraction.

153. Six minutes after dividing the aorta, the whole of the abdominal viscera were removed. There could now be no spinal action. Irritation of any portion of the intestine occasioned local, and spreading muscular contraction; but no contraction of any distant portion of the alimentary canal.

154. Two feet in length of small intestine were separated from their mesentery, which was cut off close to its attachment to the bowel. This portion was loosely spread out on the table, being still connected with the rest of the intestines at each end. As soon as the contraction produced by the manipulation had quite subsided, a single point of the loose intestine was scratched with the scalpel. At the part irritated, strong contraction ensued, and spreading from this, a twisting and coiling movement of the intestine gradually extended in each direction until it implicated the bowel to which the mesentery was still adherent. Here the ganglia being removed, no central nervous influence could operate, yet the effect as regards muscular contraction was as considerable, excito-motory action excepted, as that which results from similar irritation when the viscera remain *in situ*. The proof that muscular contraction is independent of innervation both in the power of contracting, and of communicating that contraction from fibre to fibre, cannot be carried further by experiment. Certainly some nervous tissue still remains in the terminal filaments distributed amongst the muscular fibres; but, if all nerve-tubes are independent of each other, and never anastomose after leaving their origins, whether they can themselves generate force, or whether they be mere conductors, they cannot certainly combine actions without the intervention of centres of some kind. After separation of its mesentery nothing but cut nerve-ends remain in the bowel. Notwithstanding the high authority of Müller, it seems impossible that these can exercise any re-action on the muscular fibre adequate to transferring irritation from one point to several distant points. If, indeed, the peripheral nerves were so joined together, and were so organised, as to constitute a kind of expanded nerve-centre capable of re-action on the tissues in which it lies, the question would be undecided; but such an arrangement of the muscular nerves at least is quite opposed to anatomy.

Anatomically speaking, all the known forms of muscular tissue

Fig. 1.

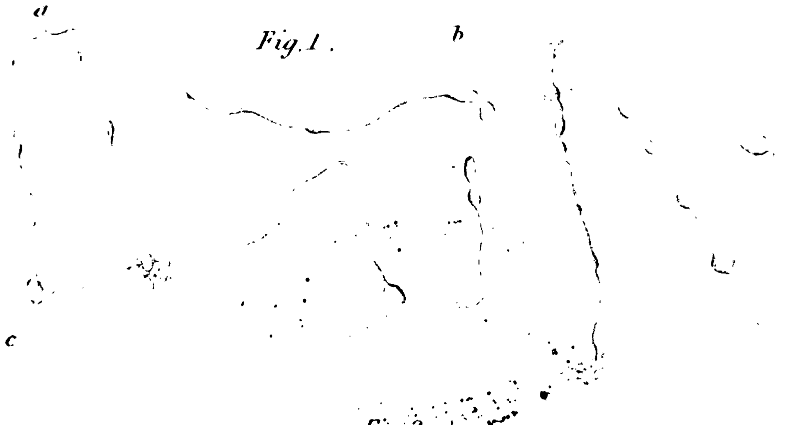

Fig. 2

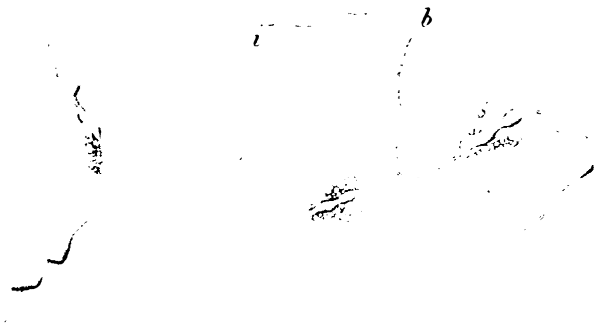

Fig. 3

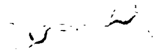

Fig. 4

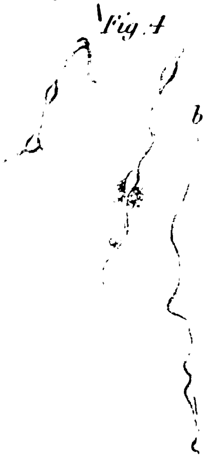

Fig. 5

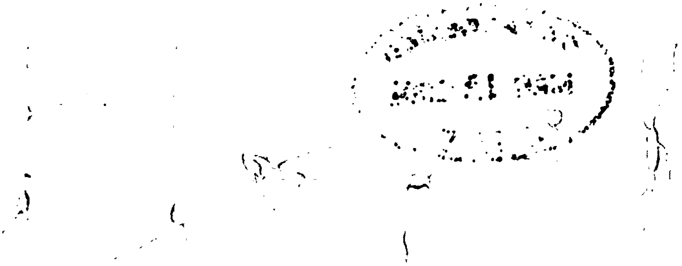

Fig. 6

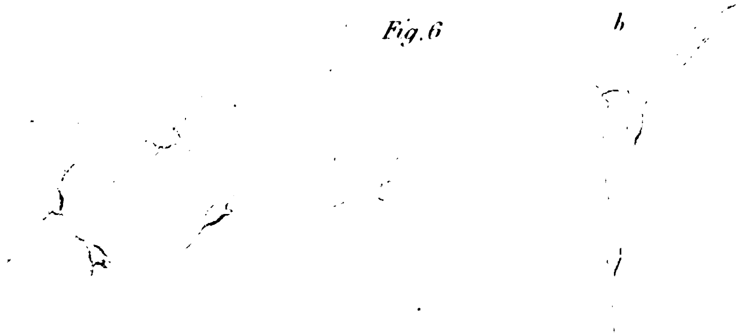

Fig. 7

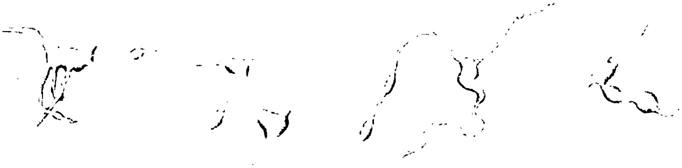

Fig. 8

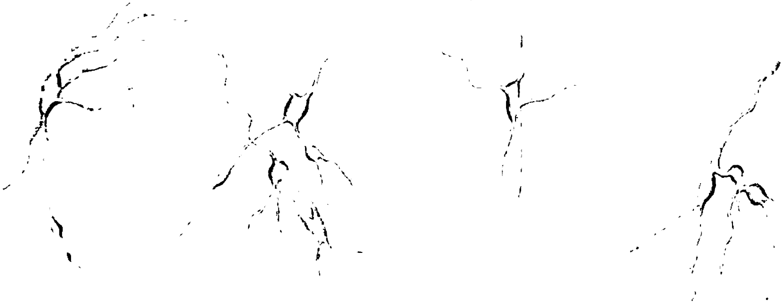

Fig. 9

Fig. 10

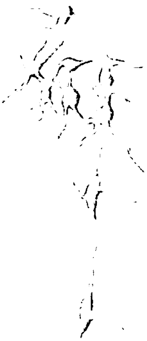

Fig. 11

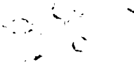

Fig. 12

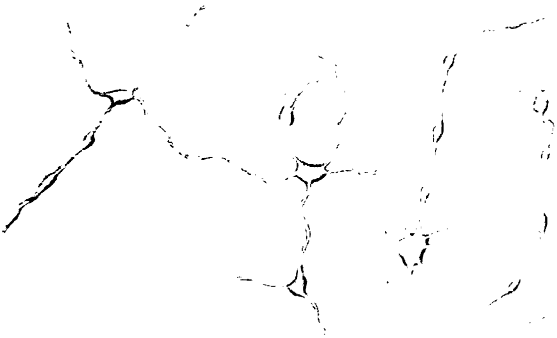

Fig. 13

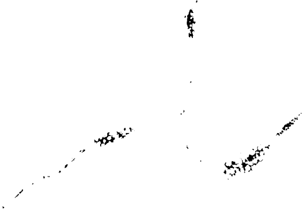

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Fig. 14.

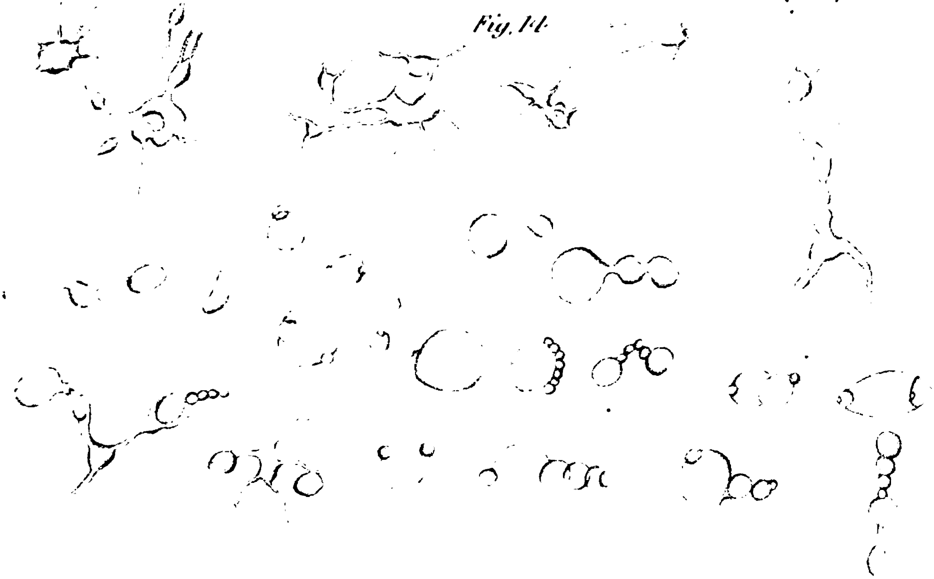

Fig. 15.

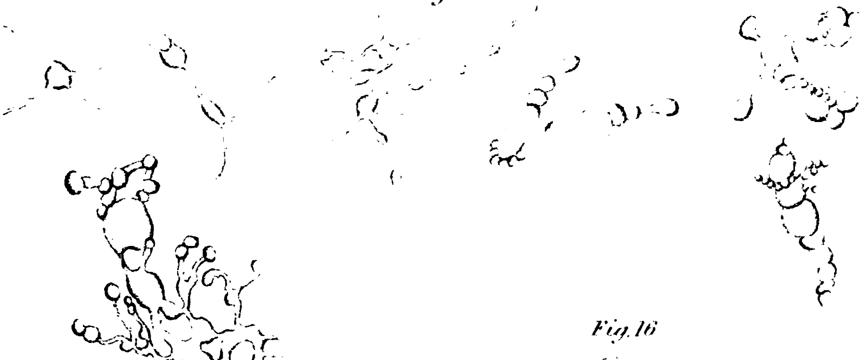

Fig. 16.

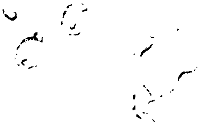

Fig. 17.

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may be reduced to two, the striped and the unstriped; but physiologically it is convenient to distinguish several varieties according to the mode in which they respond to irritation. The lowest variety is presented in the skin, where the contractile tissue is so diffused as not to be clearly demonstrable except where it exists in greater quantity, as in the dartos. To this the natural stimulus is cold. It may be questioned whether the effect of mental emotion in producing *cutis anserina* is not in part due to the actual diminution in the heat of the surface; witness the chilliness which accompanies that condition. The final cause may be that of checking further evolution of animal heat from the surface, by lessening cutaneous transpiration. It is interesting to find that mere exposure to cold will directly induce contraction of unstriped muscular fibre. Volkmann considers that he has proved this by laying bare the urinary bladder of a living dog, when that organ immediately contracted and expelled its contents. But this experiment is not satisfactory. The pain and the fright might have caused micturition. The occurrence of contraction when the animal is insensible is a better proof. The contraction induced by cold in the diffused contractile tissue of the skin is persistent. It is not known that similar action can be induced artificially by experimental irritation of any part of the nervous system.

The next remove, perhaps, is seen in the muscular walls of ducts and vessels. Their contraction during life is probably of that continued, regular character which accommodates their calibre to the distending force of their contents. When they do respond to mechanical irritation at all, the fibres in the neighbourhood of the part irritated contract and retain their contraction for some time; they never present waves of contraction, like the intestines; nor usually do they furnish a series of rhythmical movements, like the heart. Dr Procter, however, states that he transmitted galvanism from "the branch of the sympathetic which joins the ischiatic," to "one of the arteries in the leg" (l. c. p. 33), with the effect of reproducing pulsation in the artery; but nothing is mentioned concerning the heart, whether it was passive or otherwise at the time. Good experiments are wanting to decide on the extent to which irritation of the nerves can affect the arteries, the heart being quiescent.

The muscular coats of the alimentary canal consist of unstriped fibres not separated from each other, and bound up into distinct bundles, but devoid of areolar tissue, and freely intermixing amongst themselves. The unstriped character of the primary fibril confers the peculiarity of gradual rather than of sudden contraction; whilst the non-isolation of bundles, and the intricacy of the mutual connection of contiguous fibres favours the spreading of contraction from one fibre to those around. The alternating contraction which we notice in a voluntary muscle, viz. a succession of equal contractions and relaxations of the same fibres under slight but continuous irritation, cannot be induced in any way. By irritating the spinal motor nerves spinad to the sympathetic ganglia, Valentin induced ring-like constriction of various portions of the alimentary canal, *i. e. persistent* contraction. By irritating the ganglia themselves, and their nerves both spinad and viscerad to the sympathetic centres, I have found spreading vermicular motion, which extended from the several points, or the single point, at which it commenced along the contiguous fibres; but never anything like alternating contraction. From direct irritation of the muscular fibres *persistent* contraction always resulted, and lasted long in proportion as the irritation was considerable. Unstriped muscles respond more readily to direct irritation of their fibres; less readily to irritation of their nerves, than voluntary muscles.

The striped fibres possess energy and rapidity of action, and aptness for excitation through their nerves, in virtue of their special organization; and a tendency to act in portions without the general spreading noticed in the intestines, in virtue of their arrangement in fascicles and their possession of distinct sheaths. This is easily observed in the thin fasciculated muscles of the neck in a recently killed ox after removal of the head. On pinching a single fasciculus, that alone will be retracted; a sharp twitch which pulls the entire muscle will cause all of its bundles to be drawn up, but unequally. Sometimes the irritation arising from cutting through the neck will cause one of the thin muscles, a hyoidean muscle for example, to alternately retract and elongate for a considerable time, no other source of irritation being present except exposure to the atmosphere. I have seen this spontaneous alternating contraction continue for ten minutes after the butcher had suspended an ox's head. In a leg just amputated, pinching the nerves will cause vigorous contraction at the instant; and tremulous vibratory contractions several times repeated after ceasing to irritate the nerve. Great irritation of a motor nerve will cause firm contraction, which sometimes ceases with, and sometimes outlasts the removal of the irritation. Hence, we can produce alternating, continued, or persistent contraction of voluntary muscles, according to the degree and kind of irritation, without such irritation acting through a nervous centre at all. But we cannot in any way induce either spreading or rhythmical contraction of voluntary muscular fibres.

The heart is a compound variety of muscle. With the striated arrangement, it has all the energy of voluntary muscle; whilst the absence of areolar tissue and of sarcolemma, and the intermixture of its fibres with each other, confer the power of endurance and of spreading contraction of involuntary fibre. The alternate contraction and rest of the heart have been variously explained. This peculiar kind of muscular action is not due to any influence derived from the brain, nor from the spinal chord, since it outlasts the destruction of both. Nor is it owing to any especially stimulating quality of the blood, for it continues after the heart has been removed and placed in water. Nor to contact of liquid at all, for it occurs when nothing but air surrounds the organ. Nor to the stimulation of the air, for it continues in vacuo. Is it due to the cardiac ganglia? Not unless these ganglia exist to an adeqnate extent in every portion of the heart, since every portion of heart sufficiently large to contain the terminations of a sufficient number of muscular fibres, to furnish fixed points for the acting ones, will spontaneously present rhythmical movement, when separated from the rest. That this experiment often fails, is no objection. Volkmann considers the fact of some portions of a frog's heart, which has been cut up into separate pieces, continuing to present these actions, whilst certain others do not, a proof that the contraction where it is presented depends on the influence of ganglionic centres. Not to insist upon the want of proof that any more ganglia exist in the contracting than in the motionless portions of heart, how does the assumption of ganglionic influence lessen the difficulty? What confers upon the ganglion the power of acting periodically and regularly, so as to stimulate the muscular fibres at one moment, and not at the next? To invest the ganglion with any such power, is merely to remove from muscle a peculiar endowment, because we cannot explain it, to nervous tissue, where we can explain it just as little.

Nor by ascribing the periodical activity of the heart solely to muscular fibre, shall we violate anything we know of the characteristic properties of other living tissues. Alternate action and apparent rest are presented alike by all. The comparative duration of each condition is but a difference of degree. But when we examine more closely we find that the apparent rest is not perfect repose; it is merely lessened action, gradually to be augmented, until sooner or later it becomes manifest by its obvious effects. The obvious manifestation we consider alone the state of action; the preparatory process, that of rest; the succession of the two, an alternation of excitement and quiescence. From the evolution of the first embryotic cell to the last vital change of dying tissue, life pursues one never-ceasing round of activity; perpetually varying in amount, observing definite laws in every variation, this activity is never for a moment entirely suspended; it ceases only with life. The changes of nutrition are never at a stand. During emaciation they are even more active than when the presence of surplus material combined with small energy occasions extra-deposit. But the material is bad, not fully elaborated, its change into tissue rapid and imperfect, its existence short, its removal early. Still, whether sound or imperfect, leading to the fulness of health or the wasting of disease, quick or tardy, the conversion of organisable matter into tissue is unceasing. In mere nutrition, the molecules having played their part, are reduced into such matter as can be taken up by absorption, and conveyed elsewhere. In secretion, under whatever form, the abstract difference in the process consists chiefly in the fact that the residue of the molecular changes is not absorbed immediately, or, when not absorbed at all, received by excreting tubes which procure its removal elsewhere. The exhalation on the free surface of a secreting membrane may be uniform as regards quantity, the secreting activity experiencing no variation. Even this is doubtful; the pleural and peritoneal halitus may be increased in precise accordance with the movements of the viscera; the serous exhalation in the encephalon and spinal chord is presumed to vary in quantity as more or less pressure on the nervous centres is required; and there is little doubt that synovia is abundant in proportion to the exercise of the joint. Mucus is furnished freely in exact ratio to the exposure of its secreting membrane to irritation; but more or less it is always being secreted. In the production of specific secretions, such as the gastric juice, bile, semen, and the rest, the constancy of their production is less obvious. Under circumstances of excitement saliva or tears are immediately secreted copiously, but that the gentle action of the salivary and lacrymal glands is always going on, needs no argument. Just so with gastric and pancreatic juices, bile &c., secreted more copiously under the stimulation to which they are periodically subjected, it is probable that they still continue to be secreted after that state of extra-excitement has passed by. We know that semen is formed and stored up for use, whilst its organs are unexcited. This is true as regards man; but it is more obvious in other animals, in which procreative energy is required to be greatly exercised. In the male guinea-pig the *vesiculæ seminales* are often immense and full of semen. We know that the kidneys are never idle; and unless the same be true of the liver, the gall-bladder would be without a purpose. It is objected that Dr Beaumont has satisfied himself that true gastric juice only flowed during the process of digestion (Grainger on the Spinal Chord, p. 141). But that Dr Beaumont himself thought the gastric juice was accumulating in the absence of digestive activity, though not poured forth, is evident from his ascribing the sense of hunger to the distension of the follicles prior to the effusion of gastric juice. Be this as it may, the admission of air through the perforation into St Martin's stomach might modify the natural condition of the follicles and lining membrane.

Nervous power is ever ready, is exhaustible, and reproducible; is considerable or deficient in proportion to previous excitement or otherwise. We find structures fitted for its production, accumulation, conduction, and distribution. Is it not being constantly formed, accumulated, and expended? The functional manifestations of nervous tissue depending upon and corresponding to the energy with which its nutritive changes are performed, vary with, and are affected by this. But though subject to the greatest difference in amount, they can be elicited at any moment. Excitement—*i. e.* undue expenditure of nervous power—is invariably followed by exhaustion. The suspension of extra-excitement for a time invariably leads to the increase of nervous energy. We have proof that the spinal chord is active during sleep; and doubtless the ganglia are equally so; but how can we imagine the brain to be otherwise than at rest, when no evidence of its functional activity is presented? Some metaphysicians assume that the mind is ever active, even when the tired body seeks repose; that we are in reality always conscious whilst asleep, but as constantly forgetful of our sleepish imaginings in our wakeful moments. Without any such gratuitous assumption, however, there is no real difficulty. When a body feels cold, it is not really devoid of heat. When it ceases to manifest apparent electric phenomena, it is not really deprived of electricity. So also when the brain sleeps, it continues to perform its nutritive molecular changes, but with an energy too feeble to permit of mental manifestations. At any time, what will lower nutritive action in the brain will induce sleep; what will excite it, awakens. Tardy circulation from a hearty meal, or apoplectic congestion, causes stupidity and sleep. Energetic circulation from wine, or in the first stage of encephalic inflammations, causes vivid intellect and watchfulness.

Many object to the use of such words as accumulation, conduction, transmission, when applied to the nervous energy, because we know nothing as to the nature of that force. On the same ground such terms should be discarded from physics, because the exact nature of the non-vital imponderables is not ascertained.

Nor, with respect to the muscles, will the assumption of a similar accumulation of muscular force presuppose any undue expenditure of resources, an error unknown in nature's works. To those who object to speak of muscular force as a distinct entity, the argument will be unaltered, if we consider that, during the apparent rest of muscle, the sarcous elements are gradually acquiring that condition which fits them for the highest manifestation of their property of contractility; this is really tantamount to accumulation of power. And the same, *mutatis mutandis*, of other tissues. If their product be not expended in due time, in the normal manner, molecular changes do not proceed unchecked and

Fig. 14

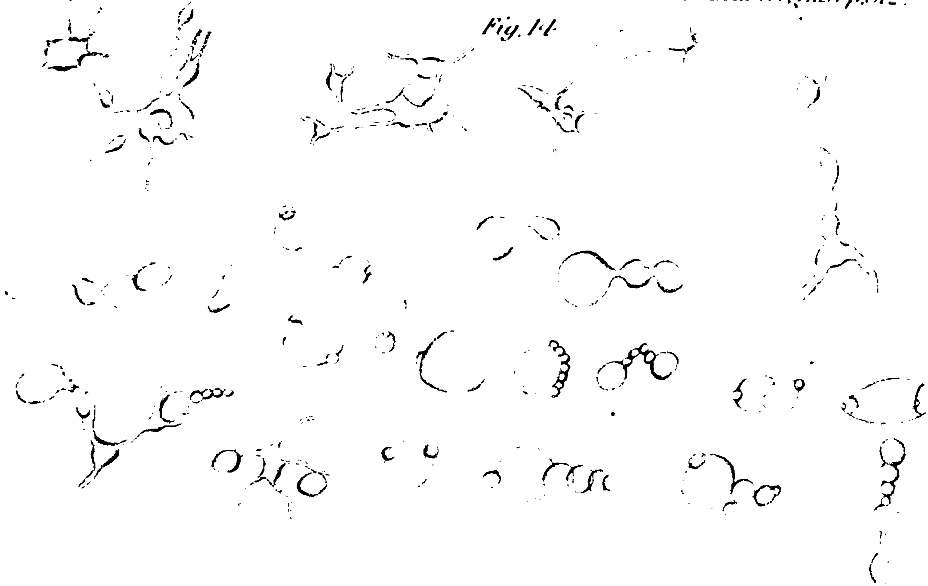


Fig. 15

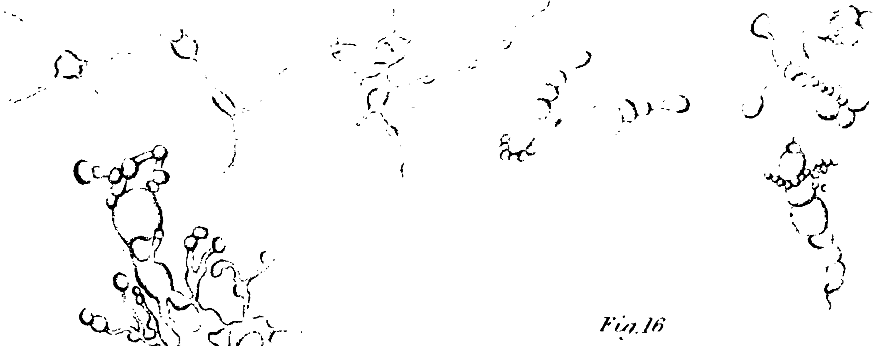
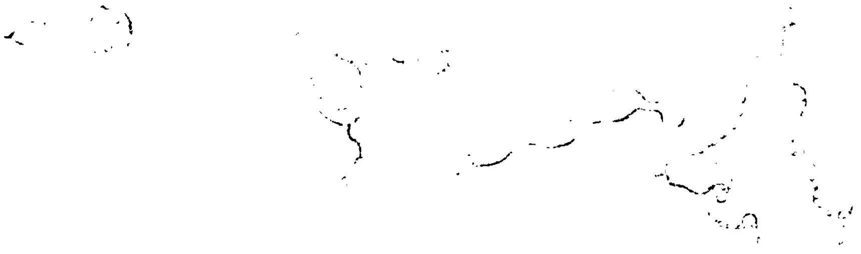


Fig. 16

Fig. 17



C. R. Hall delt.

Geo. Aikman Sc.

In grave lesion restricted to the brain, although spinal excito-motory action is everywhere proceeding, the *sphincter ani* is lax. As the spinal centre is active, why, if the state of the sphincter depends upon that, is it not close and contractile on being touched? The causes may be two-fold. It may proceed in part from the muscle being deprived of the physical re-action of sensational nerves, in part from the abolishing effect on muscular contraction always occasioned by great shock, and which always operates most considerably on parts supplied with cerebral nerves, and therefore is manifested more by the *sphincter ani* than by the purely involuntary muscles. In a similar way may perhaps be explained certain experiments of Dr M. Hall, in which sudden removal of the chord caused general muscular relaxation, without our admitting with that able physiologist, that they prove the entire dependence of muscular tone upon the spinal centre. The MM. Weber have found that a strong current of electricity transmitted through the cardiac nerves caused immediate cessation of the action of the heart by palsy; the texture of the heart being relaxed, and all its cavities full. A less strong current enfeebled the pulsations. A still gentler current accelerated them. In the last we have an exciting stimulus; in the first, overwhelming shock. The difference in the effect of severe injury inflicted suddenly, and the same amount inflicted gradually, is well known, though not always sufficiently borne in mind when performing severe surgical operations. Whatever kills instantaneously, kills by shock. A blow on the head, epigastrium, or testicle, intense emotion, electricity, and prussic acid, when they produce instant death, do so in the same manner, by shock. Every vital process is suddenly brought to a stand; the heart ceases to beat, the lungs to breathe, the alimentary canal to propel its contents, secretion stops, and the condition of the blood is altered. To what kind of influence can these effects be ascribed? As muscular contraction and secretion are essentially independent of innervation, the mere suspension of nervous influence should not instantly stop their exercise. If the blood derive no special endowment from the nerves, its state should undergo no immediate change from mere want of nervous supply. The first to suffer in shock is the nervous system; all parts of it, brain, spinal chord, and sympathetic, at once; the second, the vascular system. The blood is prevented from passing through the capillaries; and the arteries and veins alike contain blood. May it be that there is a sudden discharge of all their energy by all the nerves at the same instant; overwhelming and deranging all the atomic actions of life simultaneously? If the shock be recovered from there always appears to be great want of power in all the nervous centres; and in experiments in which the first effect of shock can be observed, it is always such as implies discharge of their force on the part of the nerves; for example, convulsions. Be this as it may, it will readily be admitted that such a considerable shock as that occasioned by suddenly removing the spinal chord may so far derange the elementary constituents of muscle as for a time to incapacitate them for performing their normal actions; but it never prevents the subsequent accession of *rigor mortis* contraction. The stiffening of the muscles after death is probably due to some such gradual accumulation of *vis insita* as that supposed, which leads at length to the last effort of muscular vitality, the *rigor mortis.* This view is supported by the fact that *rigor mortis* comes on earlier and lasts a shorter time in the more energetic muscles of warm-blooded animals; and in man, its occurrence in the different muscles, *cæteris paribus*, accords precisely with the irritability possessed; occurring early and soon over in the left ventricle of the heart; occurring later and lasting longer in the intestines. In this, the cause of *rigor mortis* exactly corresponds with the natural contractility of the different muscles. And have we not an analogy to such a gradual accumulation of force during the last moments of life, in other tissues? How else can we account for the increased clearness of mind which often immediately precedes death,—the brightening flicker of the expiring flame; or, for the occasional absorption of dropsical effusions in the last moments of life; or, for the strong convulsions which often wind up protracted dying?

The general harmonization and balancing of the action of antagonist muscles, which, without our attention, maintains steadiness of frame, and which is never entirely lost during life, is probably an excito-motory function, partly of the sensational centre, and partly of the spinal chord. Removal of the cerebro-spinal axis would annihilate this, and therefore, even if it had no effect in lessening the contractility of individual muscles, would destroy that firmness and determinate attitude of limb which results from groups of muscles acting consentaneously. It could scarcely be otherwise than that the nerves should have a great influence over the tone of muscle, when we consider that this phenomenon, under whatever name designated, whether tonicity, tonic force, insensible organic contractility, or passive contractility, is simply a low amount of exercise of that same muscular force, which, when so increased as to produce obvious motion, is greatly under the influence of the nervous system. But more than this is not proved; and if the spinal chord be not the source of tone to spinal-nerve muscles, there is no argument from analogy to sanction our supposing that the ganglia supply tonicity to ganglionic-nerve muscles.

Muscular tissue then offers no exception to the universal law of perpetual activity. The skin always accommodates itself to the surrounding temperature, or otherwise, (and virtually this is the same) it is altered by increased sensitiveness of its nerves. The hollow tubes are antagonised by the distending force of their contents, and never cease to oppose this. Voluntary muscles ever manifest the subdued contractility which enables them to antagonise each other. Now as the contraction of an entire muscle in action is not really, as it appears, uniform through the entire muscle, but consists of certain fibres contracting and others relaxing so generally, that by a resolution of forces the gross effect is an apparent contraction of all the fibres at the same instant, is there not evidence that muscular force is generated, accumulated, and expended, just as we have supposed every vital force of other tissues to be? If so, the peculiar modification of muscular contraction presented by a given type of fibre, will depend upon the fitness of the molecules of the fibres ("sarcous elements") for undergoing the requisite atomic changes more or less rapidly. Thus, if the sarcous elements of the tissue of the heart be such that they perform their atomic changes more rapidly and simultaneously than those of voluntary muscle, bearing in mind that the heart has not an equivalent antagonist to require the unperceived expenditure of its contractility, as each voluntary muscle has, some such series of spontaneous contractions as we actually have hypothetically should result. As we can disprove the dependence of the rhythmical contraction of the heart upon any portion of the nervous system, upon the blood, respiration, and every other extraneous influence; and as the possession of such an endowment by the muscular fibre, although unproved, is in strict analogy to the rest of the economy, and especially as it appears but a modification of contractility as witnessed in other muscles we may conclude that the heart alternately contracts and relaxes in virtue of the special organisation of its muscular fibre *per se.*

The cause of the sequence observed in the contraction of different parts of the heart is a question perfectly distinct from, though not unfrequently confounded with, the last. The heart acts in the order in which the blood is received into its cavities, the auricles contract first, the ventricles second. As the heart presents a greater aptness to respond to mechanical impression than any other muscle, the sequence observed is undoubtedly owing to the order in which the blood comes in contact with the several component parts. But one phenomenon essential to the result cannot thus be explained. On removing and emptying a heart still irritable, after temporary quiescence, either ventricle may be the first to contract, or either auricle. When the irritability is great, the corresponding portions, the two auricles, or the two ventricles, invariably contract together; but as irritability departs unequally, at length the right ventricle, and last of all the right auricle will contract singly. Still under any circumstances the auricle and the ventricle never contract at the same instant. Either may contract feebly several times without the other, or the ventricle may contract once, and be followed by a single contraction of the auricle, reversing the normal order. The parts which contract simultaneously are not separated by any non-muscular tissue. The parts which never contract together are. On separating the auricles from the ventricles, all may still contract, but the order is destroyed. Observation of a portion of intestine in which the contraction spreads from one fibre to another, shows that the fibres first irritated excite those around them with which they intermingle by the traction produced through their own movement. In the same way, probably, will all the fibres of the auricle re-act upon each other, but being unmixed with the fibres of the ventricle, they affect these only through the medium of the traction exercised on the tendinous ring which constitutes the common point of junction between auricle and ventricle. This being a less rapid mode of transmitting irritation than that which exists between fibre and fibre, causes contraction of the ventricle subsequently to that of the auricle, and v. v. And if the movements of the auricle be too feeble, no contraction whatever of the ventricle results, until a series of auricular contractions have occurred to make up by number what individually they want in force. In support of this, it is much more common for the auricle to contract several times to produce one action of the ventricle than the converse. This is what we should expect. The ventricle being the stronger muscle will more readily re-act through the tendon upon the auricle than the auricle upon it. The great facility displayed by the fibres of the heart in responding to mechanical impression, and in re-acting upon each other, obviates a difficulty felt by some physiologists in explaining how the application of blood to the lining membrane of the cavities can instantaneously occasion contraction of the external fibres, unless through the intervention of ganglia. The instantaneousness of contraction is not real. The fibres of the heart contract as progressively as do those of the intestines; but the progression is much more rapid.

In reference to the *ultimum moriens*, it is interesting to remark that the part which is last to lose its irritability is the venous heart. Cold-blooded animals maintain muscular irritability the longest, but their muscular energy is weakest. Hot-blooded animals have greatest energy, but the smallest power of retaining muscular irritability. It is urged that we have no proof of the hot-blooded arterial heart being more energetic than the other. But as the ventricles go through the same amount of contraction in the same space of time, must not each fibre of the larger muscle possess greater energy than that of the lesser muscle to effect this? Has the nature of the blood constantly bathing the internal surface of the cardiac cavities any influence on the contractility of the walls? There is at least a coincidence in the fact of the slow-acting involuntary muscles being invested with mucous membrane, in which the minute veins exceed the minute arteries in a far greater proportion than elsewhere.*

If these views be correct, the action of the heart is essentially independent of every nervous centre. The facts cited to establish the contrary, merely prove that it is greatly influenced by any and every nervous centre; a truth of which every man's personal experience will sufficiently convince him. Take, for example, the experiment on which Brachet laid great stress, viz. that after removal of the cardiac ganglia the heart ceased to beat: from which he concluded that the normal action of the heart was due to an influence supplied by the ganglia. With equal justice might it be ascribed to influence derived from one of the extremities, because a suddenly crushed limb will occasionally induce a similar result. It is the commonest of all errors in experimental physiology to infer that because A can be made artificially to affect the normal action of B, of necessity A must furnish something essential to the normal action of B. In illustration we may refer to the experiments on the encephalon, particularly on the cerebellum.

The continued existence of frogs after total destruction of brain and spinal chord; and the extensive movements of the hollow abdominal viscera when removed, have been adduced as good examples of muscular motion presided over by ganglia. (Mr Paget). The first, like the nutrition of imperfect fœtuses, positively proves that the brain and spinal chord are not essential to the performance of the vital processes; it by no means proves that the ganglia are. The only indication of the influence of a nervous centre being exercised in producing muscular movement, consists in obvious motion of one part induced by irritation of another and distant part, no direct and continuous muscular or other mechanical movement being communicated from the one to the other, nor any motor nerves destined to the moving part included in the portion irritated. Such an indication cannot invariably be obtained from either the heart or the abdominal viscera whilst *in situ ;* but it sometimes can. (§ 124). From these organs eviscerated it never can. Irritation of one portion of intestine, when the whole have been removed together, will by spreading contraction gradually implicate the rest; irritation of a nerve-trunk may affect simultaneously several distinct portions of intestine; and rough irritation, such as by pinching or pulling, may disturb and thus excite several distinct portions together; but these sources of fallacy considered, the fact is as stated.

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* Mr Turner of Manchester some years ago was in the habit of exhibiting engravings which demonstrated this anatomical fact very beautifully; and which I understood it was his intention to publish.

The importance of deciding whether the ganglia are motor nerve-centres to the involuntary muscles; and the great preponderance of authority (Bichat, Müller, Volkmann, Paget, Grainger, and many others,) favourable to the affirmative, rendered it incumbent upon the holder of a different opinion to discuss the subject at length. I have therefore endeavoured to show that, from general considerations, (see last paper), it is improbable that the ganglia are motor centres for muscles; from anatomy, that they are not fitted for such a function; and from experimental physiology, that they have not been proved to possess it.

If then the ganglia be, as I believe, centres for presiding over non-nervous and non-muscular organic processes, and for these only, the investigation of the function of a given ganglion resolves itself into an analysis of the different nerves which enter into its composition, and of the tissues to which its nerves are finally distributed.

Holmes Chapel, Cheshire,
August 14*th* 1846.

Art. VI.—*Surgical Cases, with Observations.* By Dr David Kerr, Lecturer on Surgery, University and King's College, Aberdeen. No. I.

I. *Case of severe injury of the left upper extremity, with fracture of the bones of the forearm, and extensive laceration of the soft parts—Amputation at the shoulder joint—Recovery.*

On the evening of the 31st December I was requested to visit L. R., a boy, 15 years of age, whose left arm had been severely injured in the morning by machinery. Having reached the scene of the accident eleven hours after its occurrence, I found the patient in a very exhausted state, in consequence of the loss of blood, and the great shock which the system had sustained. His face and lips were pale, his skin cold, his pulse feeble and rapid. The injured limb presented a truly shocking appearance. The bones of the forearm, nearly separated from the arm, were both fractured, and the soft parts most severely lacerated, the brachial artery was torn, and the whole of the humerus from its lower extremity almost to its head completely laid bare, being not only stript of the integuments and muscles, but even denuded of periosteum.

It immediately appeared to me a case requiring amputation at the shoulder joint. On inquiry, however, I found that a different opinion had been entertained by those who had previously seen the case, and that a proposal had been made to amputate the limb immediately below the joint, and as a step preliminary to the removal of the limb, it was farther proposed to cut down upon and secure the axillary artery by ligature. This latter proposal would have been no trifling matter amid the dim light afforded by a solitary candle, and all the disadvantages by which the case was surrounded. It was abandoned, however, for better reasons than its difficulty, it being feared that the patient would in all probability perish on the table under the infliction of a double operation; and as to the supply of blood to the limb, the circulation could be as completely commanded by pressure above the clavicle as by ligature of the vessel. Indeed, where a good assistant can be procured, I uniformly prefer arresting the circulation by manual pressure to applying a tourniquet, which always causes an extremely congested state of the veins, and when unskilfully applied, frequently fails in arresting the flow of blood through the arteries. This is a circumstance never to be disregarded in such a case, where every drop of blood that can be saved is of supreme importance, and may make all the difference between success and disappointment to the surgeon,—safety, and destruction to the patient.

In an attempt to save any portion of the humerus in this case, even the smallest, the bone must have been divided at a point where it was denuded of periosteum, at least posteriorly, where it was surrounded by contused and lacerated muscles, separated from the common integuments, and moreover the flaps must have been in a great measure formed from parts where, in consequence of the compression of a tourniquet, no circulation had been going on for nine or ten hours. Parts in such a condition were but ill calculated to unite by the adhesive process. On the contrary, there was much reason to apprehend the supervention of suppuration, or mortification and exfoliation of bone, with all their evil consequences, and as there was no advantage to counterbalance such a risk, amputation at the joint was preferred and performed as follows.

The patient being placed in a position as favourable for the operation as his exhausted condition would permit, and the subclavian artery compressed by an assistant, I grasped the limb with my left hand, and formed a posterior flap by cutting from without inwards. The second incision laid the joint freely open posteriorly. Reversing the knife, and at the same time drawing the arm a little across the chest, I cut round the head of the bone. This done, I abducted the arm and made the knife cut its way out on the anterior surface, forming a flap similar to the first. In this manner the limb was removed in as short a time as would suffice to detail this brief description of the operation, and with the loss of an incredibly small quantity of blood. The axillary artery and circumflex branches being secured, the sides of the wound were brought together, and retained in apposition by stitches, and a few slips of adhesive plaster, which, with a light bandage as a covering, completed the dressings. Although the quantity of blood lost during the operation did not exceed an ounce, yet so much was the patient previously reduced, that the loss of even this small quantity, and the shock of a second severe injury, rendered the pulse imperceptible at the wrist. He was immediately put to bed with a low pillow, warm applications being applied to his extremities, and wine allowed at intervals during the night.

At three o'clock in the afternoon of the day following, reaction had taken place; the natural heat of the surface was restored, the pulse was quite perceptible at the wrist, but weak and rapid. Having passed an uneasy night, and his stomach being irritable, he was ordered an opiate, which procured him a sound sleep. From this date he improved rapidly under the influence of a nutritive diet, and a liberal allowance of wine. On the seventh day, the dressings being removed, and the stitches cut, the sides of the wound were found completely united except at one point where adhesion had been prevented by the ligatures, the last of which was not detached till the 6th of February, being not less than 37 days subsequent to the operation.

II. *Case of extensive contused and lacerated wound, exposing the bones entering into the articulation, and laying open the cavity of the right knee-joint—amputation—recovery.*

A strong healthy man, aged 25, when engaged in his usual employment as a carter on the forenoon of the 30th of July last, was accidentally thrown down before the wheel of his cart, and had his right knee joint most severely injured. On visiting him six hours after the accident, I found a contused and lacerated wound on the outer side of the joint, seven or eight inches in length, with a portion of the integuments removed, exposing the bones entering into the formation of the articulation, and laying its cavity so extensively open, that the fingers could be readily passed into the joint, which was partially filled with earth. The injury had been caused by the wheel of a heavily laden cart passing partially over the joint, and slipping from off the external condyle or outer edge of the patella. As there was no serious loss of blood, the patient had recovered from the immediate shock of the injury. His pulse was good, and heat of skin natural; but his thirst was already incessant, and the injured part painful and considerably swollen. The nature and extent of the injury precluded the possibility of saving the limb; but he would listen to no argument urged in favour of immediate amputation, declaring, that he would rather die than lose his limb. The parts were accordingly brought as nearly together as the nature of the injury would permit, and fomentations applied. In less than two hours, however, his sufferings had become so intense that I was sent for to amputate the limb.

The artery being compressed against the pubis by Dr Redfern, I transfixed the thigh and removed the injured part by anterior and posterior flaps, as near to the condyles as the state of the soft parts would permit. The femoral artery and one muscular branch only were tied; and as all oozing of blood soon ceased on the application of cold, the flaps were adjusted, and retained in their position by a few stitches, supported in the interspaces by slips of plaster. A single fold of wetted lint covered the whole, and completed the dressings. The stump was afterwards kept cool by the assiduous application of cold water.

The farther history of this case may be briefly summed up. The result was most satisfactory. For six days the stump continued free from pain, or any feeling of uneasiness, the pulse never exceeding 84, and the heat of skin almost natural. During this period, his diet consisted of a soft biscuit and a cup of tea for breakfast, and the same at night. On the fifth day, the stitches were removed, as they had begun to show some degree of tightness, consequent on the natural actions going on within the part. On the sixth day, it was found necessary to administer a dose of opening medicine, and the motion unavoidable on this, or some act of imprudence on the part of the patient, disturbed the wound, and threatened to prevent its union. At my visit on the seventh day, I found he had passed an uncomfortable night, with pain and occasional starting of the stump, which now showed a very considerable degree of tumefaction. And although the pulse had been for two days at 72, it was now strong, bounding, and incompressible. The skin was hot and dry, and the face flushed. The femoral artery leading to the stump was beating violently, and the stump itself throbbing to such a degree, as to lead me to believe that suppuration was about to take place. As the patient had lost an exceedingly small quantity of blood during the operation, I ordered twelve ounces to be taken from the arm, and small doses of tartar emetic to be given at short intervals. The beneficial effects of this treatment were apparent on the following, the eighth day. He had passed a good night; the pulse had lost its unnatural qualities, and the skin its heat and dryness. The stump was reduced to its former quiescent state, free from pain, with the tumefaction much reduced. The plaster was now changed and the wound found universally adhering. The ligature on the femoral artery, however, was not detached till the 24th day.

This case affords another satisfactory proof of the superiority of manual pressure, as the best means of commanding the circulation. For this purpose no instrument can ever equal the thumb of a steady and intelligent assistant; and no other should be about the patient on such an occasion. But when I hear of several assistants all making pressure on different parts of the artery at the same time, the blood, nevertheless, flowing in jets about the room, however I may deplore the fate of the unhappy patient who falls a victim to such blundering, it is impossible not to exclaim with the poet,

> "Spectatum admissi risum teneatis, amici?"

This case affords also a good illustration of the happy effects of water-dressing, so constantly enforced by the late Dr Macartney in his surgical lectures, and so fully expounded afterwards in his excellent treatise on inflammation. I consider likewise that the absence of all undue constraint, still too much practised in the system of bandaging, contributed, in no small degree, to the successful termination.

It is stated on high authority, that "it has been uniformly found by experience, that persons who have suffered confinement to bed, have been bled and purged, and had any general excitement of the system, whether physical or moral, allayed, bear surgical operations better, and recover more quickly than those who, in the fulness of rude health, are obliged by sudden accident to undergo them." If we are to be guided, however, by the weight of evidence, it will be found to preponderate largely on the side of primary amputations, as being more successful than those performed at the secondary period. I am not ignorant of the advantages arising from the system being in good working order, whenever a patient is to become the subject of an important operation. But there are other influential circumstances, which may mar the results of our best devised and most skilfully executed operations. Had the two patients, whose cases I have related, been placed in the tainted atmosphere of an unhealthy hospital, I should not have expected the satisfactory results obtained. Or, had the treatment, employed in the first case, been resorted to in the second, instead of adhesion taking place, I should have expected the stump to have become an extensive suppurating sore. And it is but too obvious that the remedial measures, found to operate so beneficially in the second, would have hastened the first to a fatal termination. In forming an estimate, then, of the comparative success of primary and secondary operations, it is impossible to arrive at even an approximation to the truth, without being made acquainted with the whole history and treatment of each case. But, judging from the results of my own limited experience, I must give the preference to the primary operation, as having been more satisfactory in my hands, than those performed on persons subdued by treatment and protracted confinement.

Art. VII.—*Observations on the Report lately published by the Health of Towns' Commission, and on the remedial measures therein proposed for England and Wales.* By Richard White Young, M. D., Edin., President of the Royal Medical Society.

In endeavouring to form an estimate of the amount of evil which flows from a defective sanatory state of the physical circumstances in which a community may be placed, we shall best attain our object by ascertaining, as nearly as we can, the extent of increased mortality to which it gives rise. If possessed of such information, we can calculate with tolerable certainty the amount of sickness produced by it, as we know that for every death occurring in such circumstances, there are at least twenty cases of sickness. Perhaps the most fertile source of poverty among the lower classes, is that occasioned by the premature death of heads of families from epidemic and other diseases, inferring besides the expenses of funerals, the loss of the wages of their labour, for the period during which they would have survived had their lives not been thus cut short, and of its amount, the rate of mortality is of course an infallible index.

It will also enable us to form a pretty correct general idea of the amount of want occasioned by the expenses attendant upon unnecessary sickness, and the loss of labour during such sickness.

Again, as much of the increased mortality among the lower classes is attributable to moral causes, such as intemperance, carelessness of human life, &c. the amount of this will, in some measure, serve us as a guide as to the degree in which a community has become morally degraded in consequence of the continued depressing effect of physical discomfort. The additional burden imposed upon any portion of the community by excessive births will be indicated in the same way; seeing that, as a general rule, births increase in number with the rate of mortality. Thus we find that sickness, the number of births, poverty, and moral deterioration, next to death itself, the most important results of a neglect of sanatory laws, are greater or less as the mortality of the community in which they occur is high or low, and we can thus easily understand why all who have directed their attention to this subject have made the amount of mortality the principal object of their inquiries.

On this subject the commissioners appointed by her Majesty to inquire into the sanatory state of large towns have collected a mass of information well fitted to rouse the attention of the public. Without entering into details, the following facts, a few of the results of that inquiry, may be mentioned, as giving some idea of the enormous amount of mortality and consequent suffering among the lower classes of this country, mainly attributable to neglect of their sanatory state.

1. It has been shown that the annual mortality of large towns is always greater, in some cases to the extent of more than 2 per cent., in the worst-class streets than in first-class streets.* In the latter it is sometimes so low as 1.40 per cent.,† or 1 in 71; while in the former it is in one instance so great as 4 per cent., or 1 in 25.‡

2. That the average age at death is considerably less in worst-class streets than in first-class streets. Thus in York it is 12¾ years less in the former than in the latter.§

3. That the average age at death of all who die is in most large towns from 15 to 35 years less among artisans than it is among the better classes. Thus in Lancaster, the average age at death among a certain class of operatives (factory hands) is 14.8, while among the "gentry, professional persons, and families" it is 49.94. In Preston, among the operatives it is 18, and among the gentry 47.‖ In the metropolis, it is 22 among

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* Report I. vol. i. pp. 203–6.

† Second Report, vol. ii. p. 75.

‡ First Report, vol. i. p. 206.

§ Report I. vol. i. p. 235.

‖ First Report, vol. i. p. 165.

the former, and 44 among the latter. In the suburban districts of Sheffield, it is 24 among the operatives who are employed in branches of manufacture peculiar to that town, 27 among tradesmen and their families, many of whom are of course employed in peculiarly unhealthy trades, and 70 (?) among the gentry. In the town of Sheffield, it is 18 among the operatives who are employed in descriptions of manufacture peculiar to it, 27 among tradesmen, many of whom are similarly engaged, and 46 among gentry, professional persons, and their families.*

4. That the average age at death of all who die above 21 years is generally from 10 to 15 years less among artisans than among the gentry. Among those employed in factories in Lancaster, it is between 22 and 23 years less than among the gentry of the same place.† In that town it is 40.65 among the factory hands, 58.62 among artisans, and 63.07 among the gentry.

Dr Lyon Playfair, in his Report on the state of large towns in Lancashire, has shown that 14,000 deaths occur annually in that county which might be prevented, and that every individual in it loses 19 years of his life, and every adult more than 10 years of life, from causes which might be avoided.

It is universally acknowledged that much of this excessive mortality is owing to causes in a great measure independent of the physical circumstances of those among whom it takes place; and it becomes a question of great importance to what extent it is attributable to causes originating in, or connected with, their physical state, and capable of being arrested by sanatory interference. In pursuing this investigation, inquirers appear, for the most part, to have confined themselves to a consideration of those causes of disease which are directly traceable to want of drainage, deficient supply of water, &c., overlooking those which indirectly flow from the same source, but are equally under our control. In the latter class may fairly be included many of the moral influences which are continually at work in our large cities, in so far as they are called into existence by physical causes. Perhaps the most active of these is intemperance, one of the most fertile sources of disease, and one which is invariably most prevalent where physical discomfort is greatest. Experience has shown that the fewer attractions the working man finds at home, the more likely is he to seek enjoyment abroad, and that in proportion as his home comforts increase, does he seek to draw his happiness from legitimate sources.

The enormous infantile mortality which takes place among the lower orders is attributable to moral influences in a great measure traceable to physical causes. Among these may be enumerated neglect of children, both in health and sickness, from intemperance on the part of the parents; the administration of opiates to children—a custom prevalent to an almost incredible extent in many parts of England, and mainly due to the carelessness of life and health, which are the invariable attendants of a low sanatory state; and inability from youth and inexperience to rear children, the result of those early marriages so prevalent wherever the rate of mortality is unusually high. It is thus evident that in estimating the degree in which physical causes contribute to the excessive mortality in our large towns, we are not to content ourselves with ascertaining their immediate and direct effect on the public health, but must take into calculation their influence in calling into existence moral evils which are in turn fruitful sources of mortality. We are thus justified in ranking under the head of preventible mortality, not only that directly traceable to physical causes, but much of that ascribable to moral influences, and in concluding that at least three-fourths of the excessive mortality annually occurring among the lower classes of this country may be checked by sanatory interference. It will afterwards be shown that excessive poverty, undoubtedly a powerful predisponent to disease, is in a great measure dependent directly or indirectly on physical causes.

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* First Report, vol. i. p. 265.

† Report II. vol. ii. p. 71.

Even were it of any practical importance to ascertain to what extent physical causes operate simply as primary and immediate agents in the production of mortality, I fear it would be impossible to do so. This has been attempted in various ways, such as by citing instances where improvements in drainage and ventilation have been followed by a marked diminution of the amount of mortality in the drained districts. In all such cases, however, it will be found that the habits of the people, and often the class of inhabitants, have been improved by the change, whence we may fairly conclude that their increased immunity from disease is owing to the removal of moral as well as of physical evil.

With the same view, attempts have been made to show that infant mortality is due to physical causes alone, inasmuch as children are exempt from vices, and otherwise less under the control of moral influences than adults, and hence, that it may be regarded as a tolerably correct criterion of the share which purely physical causes have in producing the mortality prevalent in any community. But it must be borne in mind, that, although in their own persons the young are comparatively little exposed to such influences, they are so to a great extent through the medium of their parents, on whose care of them in health and sickness, as regulated by the intensity of their parental feelings, and by their character and habits,—their safety must depend until they are able to provide for themselves.

The plan has also been tried of comparing the mortality of two districts inhabited by the same class of people, and which present no feature of dissimilarity, except that the one is well drained and ventilated and the other is not, whence any difference in the rate of mortality can only be accounted for by the difference in their sanatory state. But this expedient has been as unsuccessful as the others, for it has always been found in such cases, that the better drained district has been inhabited by a much more respectable class of tenants than the other, although coming under the same general head as artisans or labourers.

Perhaps the most fatal and calamitous form which disease takes among the lower orders, is that of fever. Its ravages are invariably more frequent and fatal, in proportion as the sanatory state of the population among whom it takes place is lower, and as, consequently upon this, destitution and wretchedness are more prevalent. Dr Southwood Smith, in his evidence before the Health of Towns' Commissioners, speaking of fever in London, says, "If you trace down the fever districts on a map, and then compare that map with the map of the Commissioners of Sewers, you will find that, wherever the Commissioners of Sewers have not been, there fever is prevalent, and, on the contrary, wherever they have been, there fever is comparatively absent." It is easy to understand that this must be generally correct, not only in London but elsewhere, inasmuch as the inhabitants of undrained districts are invariably the least favourably situated in all respects as to their sanatory state, and are consequently the poorest and most degraded.

The question, whether or not the effluvia arising from decaying animal matter be capable of producing fever, is one of little practical importance. Although it must be admitted that no satisfactory evidence has yet been adduced to show that fever may arise from this cause alone, we are not hence to conclude that sanatory measures which should provide for the speedy removal of such matter, would not have great effect both in directly diminishing some of the concurrent and accessary causes of fever, and in checking its progress when it has once made its appearance. Sanatory improvements are, on the contrary, quite consistent with the fullest acknowledgment of the theory so ably borne out by Dr Alison, that the disease is to be traced to the mental and physical depression arising from "the destitution and irregular mode of life" prevailing among those who are most subject to its attacks; seeing that a defective sanatory state is confessedly one of the most fertile sources of such intemperance and destitution.

Besides, it is admitted that fever, however it originates, is propagated much more rapidly and surely in localities where the contagious emanations arising from the bodies of those suffering from it are concentrated, by the deficient ventilation which almost invariably accompanies imperfect drainage, and among persons whose constitutions are enfeebled by the habitual respiration of vitiated air in ill-drained and ill-ventilated situations.

Mr Chadwick has calculated that the annual mortality from typhous fever alone in England and Wales, is greater than the loss sustained by the allied armies at Waterloo, and as that disease is only fatal in about 1 in 12 of those attacked, we may conclude that about 12 times the number of those who fell on that occasion are annually affected by it. But what renders the mortality arising from this cause much more calamitous than that proceeding from any other, is the circumstance that the period of life during which the constitution is most susceptible of its attacks is that between 20 and 40 years, or in other words, that in the great bulk of fatal cases, a family of young children are left without their natural protectors. The expense entailed on the public from the number of children thus thrown upon the parishes for support in consequence of deaths, most of which might have been prevented, is enormous, and yet forms but a small portion of the evil.

The amount of disease directly traceable to defective ventilation or impure air alone is very great. To this must in a great measure be attributed the rapid spread of infectious diseases among the poor, when they have once, from whatever cause, made their appearance. It has been found that men employed in-doors are more liable to consumption than out-door labourers, although in the receipt of better wages, and hence in the enjoyment of a greater degree of physical comfort. As this circumstance can only be owing to the want of pure air and exercise, and as the want of exercise alone does not, as shown in the case of literary men, exert the same prejudicial influence, it has been argued that the respiration of impure air in their crowded and ill-ventilated workshops is its principal cause. In confirmation of this view, it has been found that in workshops constructed with two separate floors, and with an engine below, from which a current of heated air is constantly rising, the number of sufferers from chest complaints is much greater, in the higher flat, where the air is hotter and more impure, than in the lower, where it is only partially vitiated.* Dr Guy found that while of 101 men having more than 600 cubic feet of air to breathe, only six were affected with pectoral complaints, out of 104 having less than 500 cubic feet, 26 were so affected.* It is the opinion of many, that, in addition to its directly injurious effect on the health, the impure air of workshops exerts a peculiarly depressing effect on the minds and bodies of those employed in them, which prompts them to the habitual and excessive use of stimulating liquors. Dr Guy is of opinion, that 5000 adults die annually in England and Wales alone from consumption, whose lives might be saved by a proper system of ventilation in houses and workshops and other sanatory measures, and of these preventible deaths, he attributes one-sixth part to the deficient ventilation of workshops alone.†

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* First Report, vol. i. p. 25.

The ruinous results of a defective sanatory state, even in a pecuniary point of view, are worthy of the most earnest attention. Among the losses sustained by the working classes from this cause, may be enumerated, the loss of the value of labour from premature deaths and preventible sickness,—the expense of funerals,—of medical advice and medicine during sickness,—of rearing a much larger number of children than are born under a better sanatory state, and a great proportion of which are not destined to reach maturity,—and of contributing to the support of the widows and children of those prematurely cut off. The annual loss sustained by the working classes in Lancashire alone, from preventible mortality and sickness, and the attendant excess of births, has been calculated by Dr Lyon Playfair at upwards of five millions Sterling. The burden imposed upon the public in the form of parish rates for the support of the families of those prematurely cut off must amount to a very large sum annually. The increased expense of life assurance, from the diminished average duration of life, must of itself form a considerable item in the annual expenditure rendered necessary by the existence of a great preventible mortality. The poverty thus created among the working classes themselves necessarily leads to over-crowding, and consequent aggravation of the physical evils which called it into existence.

The inevitable effect of a low sanatory state on the moral and religious character of a community is too forcibly illustrated by the scenes of degradation and vice, which every one of our large towns presents, to require a single remark. The process of moral deterioration is one which is rapidly advancing, and is raising up in the midst of us a vast heathen population, whose vices are at once the opprobrium of civilization, and a fearful testimony against the apathy that nursed them into existence. Nor does society at large, to whose neglect these evils are mainly owing, go altogether unpunished.

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* First Report, vol. i. p. 97.

† First Report, vol. i. p. 93.

A heavy tax is laid upon it, and one which is constantly increasing, in the shape of hospitals, charitable institutions, and parochial aid, for the purpose of palliating misery, which it has never had the resolution to prevent; and it is ever more or less exposed to those epidemics, whose favourite seats are the homes of poverty and filth, but which every now and then trespass their natural boundaries, as if emphatically to remind it of its neglected duties and forgotten responsibilities.

That well conducted sanatory measures might diminish the surplus mortality of large towns to a great extent there can be no doubt. This they would effect in the first instance, by removing many of the immediate physical causes of disease, and afterwards by elevating the moral state and improving the circumstances of the lower classes. Intemperance and poverty, two great causes of disease, have just been shown to be in a great measure attributable to a low sanatory condition, the former being the immediate and almost inevitable effect of such a state of things, and the latter being partly the result of intemperance, and partly of the loss and expenditure attendant upon a great amount of preventible sickness and mortality. It is only, then, what we are fairly entitled to expect, that on the establishment of adequate sanatory provisions, intemperance and poverty should diminish to the extent to which they have been induced by a neglect of sanatory laws, and that a corresponding decrease in the amount of mortality arising from these causes should take place. Nor is there any reason to doubt that, in the course of time, and with the aid of a well-directed moral agency, the circumstances, habits, and physical condition of the poor may be so much improved as to render their sanatory state in no great degree inferior to that of the better classes.

The effect which a sanatory enactment would have in increasing the funds at the disposal of the poor, is of itself a consideration of vast importance. The saving consequent upon the decrease of intemperance alone would be sufficient to increase incalculably the comforts of the working man; and were we to add to this, what is now utterly lost to him in wages and expenditure by preventible mortality and sickness, and by excessive births, a complete revolution would be effected in his social condition—a consideration which at once silences those objectors who tell us that it can be of but little use to ameliorate the physical condition of the people, if we do nothing to improve their circumstances. In short, it may not be too much to hope that, should extensive sanatory measures be adopted, suffering from abject poverty will, in periods of average national prosperity, become comparatively rare, while, when it does occur, society being to a great extent relieved of the burden now imposed upon it for charitable objects, and having more accurate information as to the facts, will be both more able and willing to afford it effectual relief.

The subject of sanatory improvements is one of overwhelming interest to the political economist, as it is only by such improvement, assisted by the general diffusion of a sound religious education, that we can reasonably hope to arrest the growth of a population which threatens soon to exceed the means of supply. It has been objected to sanatory measures, that they are calculated to check the progress of diseases which have been wisely called into existence for the purpose of keeping under a redundant population, and preventing an alarming multiplication of the human species. A careful examination of facts, however, shows that a low sanatory state, so far from acting in this way, invariably leads to increase of population, which increase is, as a general rule, more rapid in proportion as the sanatory state is more defective. It is an established fact, that the number of births in any community invariably increases with the rate of mortality. This is evidently owing to the want of forethought and moral restraint characteristic of the working classes—always more remarkable as they are lower in the social scale, and leading either to early marriages, or where these are less common, to a great amount of illegitimate births. Among the working classes generally, and in manufacturing towns in particular, marriages are too often contracted immediately after the parties are capable of earning the wages of adults, whence their numerous and burdensome families. But a high mortality has also a direct effect in increasing population, as from the great mortality prevalent among children during lactation, the families of working people are for obvious reasons frequently larger than they would otherwise be. So great is the increase in the number of births from these causes, that while the births occurring in a population where the mortality is very high, or in other words, where the sanatory state is low, are much more numerous than those which take place among the higher classes, the number of children actually arriving at maturity is greater among the former than among the latter, notwithstanding the enormous infantile mortality to which it is exposed. If, then, the number of births, as a general rule, rises and falls with the number of deaths, we are fairly entitled to conclude, that sanatory measures would diminish excessive births in the same ratio as they diminish excessive deaths,—effecting this object by elevating the tone of moral feeling among the lower orders, and leading to habits of providence and self-restraint. The experience of Geneva, where a system of registration has been in existence for three centuries, is fully confirmatory of the correctness of this conclusion. The tables of that city show distinctly that for ages past the number of births has been diminishing as civilisation has advanced, and the chances of life have been increasing. In 1836 the births barely replaced the deaths.

Even were we to put out of view altogether the primary objects of sanatory interference, the considerations that we should thereby be enabled to increase to an extent of which we can at present only form an approximative idea,— the wealth and comforts of the working classes, and, to a great extent, to check the rapid increase of population, and particularly of a population such as exists in the most unhealthy parts of a country, an undue proportion of which is infantile or unable for work, and the whole liable to numerous diseases,—objects which have hitherto baffled the ingenuity of political economists, and the wisdom of statesmen,—would of themselves be sufficiently powerful to enforce it upon the legislature.

In order to form an idea of the degree in which the moral and social state of the lower orders would ultimately be benefited by improvements effected in their physical condition, we have only to consider how much it has been deteriorated by the continued operation of physical disadvantages and the neglect of sanatory laws. Few who have come much in contact with the lower orders in our large towns can entertain a doubt that no moral agency, however vigorously worked, can effect the great object of regenerating the masses of the people, unless they be first placed in circumstances of tolerable physical comfort; nor is this opinion inconsistent with the admission forced upon us by numerous examples in our own, and particularly in other countries,—that the lowest moral state may coexist with the most favourable physical condition, since this only shows that the moral element is indispensably requisite to complete the transformation.

The inadequacy of existing provisions for the salubrity and comfort of our town population, and in many instances the entire absence of any such provisions, are attributable to a variety of causes, which have been carefully elicited in the recent sanatory inquiry. The drainage of towns has never been made the subject of comprehensive and enlightened legislation. All drainage acts, including the highway act and the various local acts, are limited in their objects and inadequate in their provisions, in consequence of which drainage is in all cases partial and imperfect, and in some towns quite unknown.

The commissioners entrusted with the administration of these acts, besides being seldom qualified for the direction of scientific works, are generally elected for a limited period, whence flow all the evils attendant upon a short tenure of office. Such bodies are also much under the control of local influences, which frequently present an effectual barrier against the execution of all improvements, however loudly called for, which are calculated to affect private interests. These circumstances so far account for the partial manner in which the limited powers afforded by the local acts have been employed for the improvement of towns. The almost universal inefficiency of works of drainage is just such as a knowledge of the unscientific manner in which they have hitherto been constructed might have prepared us to expect. The evidence amassed by the Health of Towns' Commissioners shows that drainage has in most towns been executed without any general survey of the area to be drained, in consequence of which neglect, serious loss and inconvenience have in many cases been entailed from the construction of sewers at improper levels; while the area for drainage, included within the jurisdiction of the local board, is almost invariably much too limited, not including the natural outlets, or even the suburbs of the town to be drained. The principles of science have never been applied to the construction of sewers, and the individuals entrusted with their immediate execution are seldom selected with reference to their scientific attainments, but on the contrary, are frequently grossly incompetent. The consequences of all this have been that sewers are generally of a form calculated to retard rather than accelerate the flow of their contents, and hence are not only highly inefficient, but are constant sources of annoyance and disease from the fetid emanations which they give forth, and which pass into houses by the ill-constructed house drains;—that they have been executed without regard to the probable future wants of the district through which they run, and at a great unnecessary expense; and that, as an inevitable consequence of the faults of their construction, they are kept up at a great cost from the expensive alterations which they from time to time require. The principal obstacles to the more general introduction of house-drainage are the imperfections of the common sewers; the expense necessarily incurred, according to the present system, in the construction of house drains, and the unwillingness of many landlords at their own expense to furnish property in which they have only a short interest, with improvements which are to be the source of permanent benefit.

The insufficient manner in which large towns are supplied with water is attributable to the expense of its introduction into houses, according to the present extravagant mode of doing so, to its high price, and in some cases, to the deficient quantity in which it is supplied, in consequence of its being furnished by public companies whose principal object is their own aggrandisement. Building, and the external and internal ventilation of houses, have never been made the objects of adequate legislative enactments, and the consequence has been, that the houses of the working classes are built almost solely with a view to economy in outlay, and often in direct violation of sanatory laws. The minor essentials to a high sanatory state have been similarly neglected, and from similar causes.

I shall now proceed, in the first instance, to point out hurriedly the different means by which it appears from the evidence supplied to the Health of Towns' Commissioners, that increased efficiency and economy may be secured in connection with the details of sanatory improvements, and afterwards to consider the best modes of carrying out such improvements as regards the administrative body into whose hands they should be entrusted, and the most advisable plan of defraying and levying the expenses of their execution.

Supply of water.—The superiority of the plan of furnishing water at high pressure and in constant supply to the ordinary mode, is strongly insisted upon in the evidence brought before the Commissioners. The advantages of that system are said to be the following.

1. There being no longer any necessity for tank, butt, and ball-cock, where there is a constant supply of water, the expense of introducing water into houses is diminished by more than one-half. Besides, water preserved in tanks is liable to become impure from soot and dust falling into it, an evil which is of course avoided under the operation of the system in question. The expense of keeping the cisterns in repair, and the space occupied by them in the small houses of the working classes, are saved.

2. The water will be readily applicable for the purpose of extinguishing fires, it being proposed to keep up such a high pressure as will throw water over the highest houses. For this purpose it has been proposed that fire-plugs with two jets each should be inserted along the mains 50 yards apart, so that no house would be more than 25 yards distant from a fire-plug, and that in the event of a fire breaking out, no farther delay would take place in applying the water to its extinction, than was necessary to attach the hose. In some towns where this plan has been adopted, it is said that fires are rarely allowed to proceed to a ruinous extent, and that the premiums on fire insurance have fallen considerably. In the case of a large manufactory or museum, additional security might be afforded by conveying a pipe through the whole building, and attaching a plug to it in every room with a hose ready to be applied. These fire-plugs may also be employed for watering the streets, thus superseding and saving the expense of water carts for

washing the fronts of houses and public buildings, and for scouring the pavements in dirty weather.

3. This system would afford great facilities for flushing sewers, for the general introduction of baths into private houses, for the establishment of public baths, for the erection of public ornamental fountains, for the irrigation of gardens in the suburbs of towns, and many other objects of public importance, unattainable under the prevalent system of an intermittent supply.

The expense of supplying water on this system will vary with the natural advantages of the different towns in which it is proposed to be introduced, being of course greater where the source of supply is not sufficiently high, and the water has to be raised into an artificial reservoir by mechanical power, or to be propelled by a force pump.

These advantages are represented as having had the effect of rendering the introduction of water into private houses general in those towns where this system is in operation, and this, by dividing the interest of the capital expended over a greater number of families, has reduced its price considerably. Thus, at Nottingham, a constant supply is afforded at the rate of 1d. a-week to each house, and a dividend of 5 per cent. is paid on the outlay.

In cases where a sufficient quantity of water, for the purpose of supplying a town on this plan, cannot be obtained from springs, lakes, or streams in the vicinity, the expedient may be resorted to, where practicable, of collecting the surface water of some part of the neighbourhood, as recommended by Mr Thom.

It is to be feared, however, that while such a mode of supplying water would be very desirable, its advocates have in some degree over-stated its advantages, and not taken sufficiently into account the difficulties in the way of its universal adoption. In large towns, an enormous body of water would be requisite, and this could not in all cases be obtained either from the usual sources of supply, as springs and rivers, or by the collection of surface water. Again, in cities such as Edinburgh, whose sites are irregular, a very great degree of pressure would be required to overcome the obstacles presented by the inequalities of the surface. It has also been found that when water is delivered by cocks without the intervention of cisterns, the expense incurred by the wear of the cocks from the attrition of the sand which almost all water, however well filtered, contains, has in a few years equalled the outlay which would have arisen from the introduction of a cistern with its accompanying apparatus at first. That the communication pipes must be much stronger and more expensive where the water is on high pressure, is undoubted, although this is denied by some of the advocates of that system.

In experiments recently performed in London with the view of ascertaining the applicability of water at high pressure to the extinction of fires, it was found that the addition of a second hose when the first was playing had the effect of reducing very materially the height to which the water rose. Should this result be confirmed, it will be evident that, in the event of a serious fire, not more than one hose could be brought to bear upon it effectually in this way, and that fire-engines would still be requisite. Notwithstanding, the great advantage would be enjoyed of speedy applicability, and the majority of fires would be extinguished before they had proceeded to an alarming extent.*

Such being the state of matters, it would be impossible to lay down any general rule as to the mode of supplying water which should be selected. This ought to depend on the circumstances of each individual case, more particularly with reference to the supply of water obtainable, and the physical conformation of the town and surrounding country.

Drainage.—The importance of a good supply of water is not to be estimated by a consideration of its value for domestic purposes only, but as being essential to an efficient system of house drainage. It is consequently of importance to ascertain in what way water, used for culinary and cleansing purposes, may be most effectually applied for the removal of refuse. The refuse of towns is at present principally disposed of either by being taken away at once by means of dust carts, or by subsequent removal from cess-pools, or from the sewers into which it passes, which are often so ill-constructed as to be incapable of conveying it away entirely. Experience has shown that no system of sewerage can be effectual which is not general, and which does not provide for the removal of all fluid and semi-solid refuse by the force of water passing through well-constructed house-drains, if necessary, branch drains, and common sewers. The most essential particulars to be attended to in such a system of drainage, as shown in the evidence collected by the Health of Towns' Commissioners, are as follow:

1. As a preliminary to the drainage of any town, it is indispensable that an accurate survey be obtained of its whole *geological area.* Such a survey will also be applicable to the laying of gas and water pipes, and the formation of new streets.

2. Where practicable, there should be a water-closet with a house drain in every tenement, in the construction of both of which there is great room for economy. It is of the greatest im-

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* Still greater security will be afforded by means of a fire-engine just brought out by an ingenious engineer in Manchester, which is in every respect so superior to those now in use, as to render it certain that it will soon supersede all of them. It is known as "White's Patent Fire-Engine."

portance that the size of the house-drain should bear a direct proportion to the quantity of water it is expected to convey, both with a view to economy, and more particularly in order to secure hydraulic pressure. With this object it should be tubular and glazed internally, as suggested by Mr Guthrie,—an advantage of which arrangement would be that the drains of several houses might be made to enter the sewer at the same point, thus saving the expense of separate openings and valves for each. If constructed on this principle, house-drains need not be more than three inches in diameter. The economy of water which would ensue is obvious.

3. The size of branch drains and sewers should also be proportioned to the quantity of water they will probably be required to convey. The greater number of the sewers in London are at present cleaned by periodically carting away their contents, for which purpose they must be made sufficiently spacious to admit a man, and be provided with man-holes at certain intervals. It has recently been proposed, and the proposal has in some cases been carried into effect, to substitute the process of flushing, which consists in the damming up and sudden admission into the sewers of a great body of water, for this very expensive and disagreeable mode of procedure. The advantages of flushing are the following;—*a.* A great pecuniary saving is effected by it in various ways; sewers constructed with a view to being cleansed by flushing may be made much less capacious than they now are, men no longer being required to cleanse them, and do not require man-holes. The expense of cleansing them, and of carting away the contents is also saved. *b.* Fetid accumulations are prevented, and their destructive emanations in a great measure, if not entirely, abolished, partly in consequence of the absence of the accumulations which give rise to them, and partly from the gases which still form being swept away by the current. It is thus expected that traps and chimneys for the ventilation of sewers will eventually be rendered unnecessary. *c.* Private drains can no longer be choked up in consequence of obstructions occurring in the sewers. *d.* The exposure of the filth during its removal is avoided; *e.* and the health and habits of the men employed in performing that duty are no longer deteriorated. It is the opinion of many, that in the event of water and private drains being universally introduced into houses, such a large body of water will always be passing through the sewers as to effect the object of carrying off their contents without any regular flushing. The oval form appears in every respect best adapted for sewers. It is that capable of bearing the greatest degree of external pressure, and which opposes the least resistance to the passage of water. Requiring less material, oval sewers may be constructed at much less ex-

pense than the clumsy flat-bottomed drains now in use. A farther saving may be effected by constructing both branch-drains and sewers of glazed fire-clay tubes, which possess the advantages of chèapness, durability, great strength, and imperviousness to gaseous or liquid emanations.

In the execution of any plan of systematic drainage, it would be absolutely requisite that the house and branch drains should be made under the superintendence and direction of the administrative body into whose hands the construction of the common sewers would be entrusted. By this means a degree of efficiency and uniformity would be ensured that could be obtained in no other way, and the works being done by contract, a saving of 25 per cent. would be effected in their construction.

In addition to the infinitely superior efficiency, which works for the supply of water and drainage constructed in conformity with the principles just laid down, would be possessed of, the outlay necessary for their execution would probably not be one-fourth of that requisite under the present imperfect system. It has been calculated that the house of a working man may be furnished with a water-closet, or an efficient substitute for one, —a soil-pipe and a waterpipe, for L.5, or if the payment be spread over a period of thirty years, at a weekly rent of 1½d. One penny a-week for water being added to this, a constant supply of pure water, and all the advantages of an efficient system of drainage, may be enjoyed at a weekly rent of 2½d.

The experience of other countries, and of some towns in our own, has suggested the propriety of applying the sewer water of towns to agricultural purposes. It is confidently anticipated that such a step would be the means of securing to the country an enormous annual revenue from what has hitherto been generally regarded as a mere nuisance, and of contributing largely to the development of our agricultural resources. An additional advantage would be, that the water of rivers near large towns would no longer be contaminated by being made the receptacles of the contents of their sewers. Mr Smith of Deanston proposes to convey the sewer water in pipes, under a pressure of from 100 to 150 feet of altitude, to be obtained by pumping to the requisite height by means of a cheap and simple apparatus of cast-iron, to convenient points in farms several miles distant from towns, there to be distributed by jet. He suggests that the pipes for this purpose should be sunk two feet under the surface, and should have a plug opening for attaching the hose for every 40 acres. Part of the sewer water might be distributed by irrigation to meadow land. He thinks that about two applications to the tillage land in the course of the season would be sufficient.

Liquid manure is much more valuable than manure in the solid form, as it sinks at once into the soil, and comparatively little is lost by evaporation. For the same reason, the emanations proceeding from it are less offensive. Indeed, at the distance of a few miles from a large town they would be productive of no inconvenience whatever. Mr Smith is of opinion that the sewer water of towns is capable of producing an annual revenue of L.1 for each inhabitant, at which rate it would yield for London alone the enormous sum of L.2,000,000 a-year.

Ventilation.—The state of our knowledge in respect to internal ventilation is not such as to justify the recommendation for general adoption of any particular plan. It is evident that the systematic ventilation of the houses of the poor must be delayed, until the simpler and more practicable departments of sanatory interference shall have been carried out; and in the meantime it is to be hoped that the subject will meet with the prompt and active investigation which its importance demands.

The principal object to be attained in the ventilation of small rooms and workshops, and indeed in ventilation generally, is to create a current of air sufficiently great to effect the desired change, and yet not so strong as to cause a perceptible movement in the atmosphere. The difficulty of effecting this is extreme in the case of the poor, who, being to some extent enfeebled by the constant respiration of impure air, are acutely susceptible of cold, and would much rather breathe a vitiated atmosphere, whose bad effects are not perceived by them at the moment, than tolerate any plan of ventilation which would lead to the slightest draught. Several very ingenious expedients have been resorted to, with more or less success, for ventilating the houses of the poor, such as the chimney ventilator of Dr Arnott, which is so constructed as to permit the vitiated air to pass into the chimney, but to prevent the return of smoke,—and the ventilating window panes of Mr Toynbee and Dr Guy. But the defective construction of the chimneys of the poor, which are frequently so large as to prevent the necessary current of air, presents a serious obstacle to the general introduction of Dr Arnott's contrivance, while Dr Guy admits that the draught occasionally created by it prevented the more general use of his. Both of these plans possess the advantages of cheapness and easy application. Much might be done in Scotland by the ventilation of the *common stairs.* These are generally very ill-aired, and their deficiency in this respect contributes much to the impurity of the atmosphere of the houses which open upon them, and which are in a great measure dependent upon them for their supply of pure air. For this purpose it would probably be sufficient to provide their windows with perforated zinc plates on Mr Toynbee's plan.

To the mode of assessment of the window-tax, has with justice been attributed much of the deficient ventilation of houses, inasmuch as it holds out a direct inducement to builders to construct houses with as few windows as possible, or, in other words, operates as a premium on defective construction. The remedy for this evil is one which might be applied without the slightest injury to the revenue, viz. to assess on the principle that every house requires a certain number of windows in proportion to its cubic contents, and to charge accordingly without reference to the number of windows which actually exist. It has been said that this question is one of no importance, so far as the poor are concerned; but it must be remembered, that although houses having less than eight windows are exempt from this tax, it is a common thing in England for several poor families to occupy different apartments in houses of such dimensions as to require more than that number of windows.

It has frequently been proposed, as a means of promoting external ventilation, that houses in the most crowded and unhealthy parts of large towns should be removed, and the spaces occupied by them converted into thoroughfares,—that cellars should no longer be tolerated as places of residence,—and that narrow and ill-ventilated streets and courts should be replaced by others constructed on approved principles. Such a proposal appears, at first sight, to furnish an excellent remedy for the evils attendant upon overcrowding, and one whose execution need only be retarded by the great expense with which it would be accompanied, from the high price of property in central situations. Farther consideration, however, will show that its rash adoption might be followed by the most disastrous results. The direct effect of forming a thoroughfare in a crowded district, without having first provided accommodation for its inhabitants elsewhere, is to cause farther overcrowding in the immediate neighbourhood, in all likelihood too densely inhabited already. A similar result will follow the clearing away and rebuilding of a crowded district on scientific principles, as such a district, in its improved state, would probably not accommodate half the number of inhabitants that it had done before. Nor is it sufficient to obviate this evil, that cheap residences should be erected in the suburbs of the town for the ejected inhabitants, artisans finding it inconvenient to reside at a distance from their places of work, and the poor being unwilling to remove themselves far from charitable institutions. In small towns this difficulty would be less felt, and in large cities it might be in part obviated, by the parishes rendering it imperative on as many of their paupers as could do so without great inconvenience, to reside in such houses in the suburbs, by which arrangement, an improvement, proportionate to the number of individuals removed, might be effected in the width and ventilation of the districts they had left. It would also be necessary, in rebuilding crowded districts, at least in our large cities and manufacturing towns, so to proceed in laying out the new streets, as that as large a number of their former inhabitants might be permitted to remain in them as was consistent with a due regard to sanatory regulations. In small towns the inconvenience felt by the removal of a portion of the population to the suburbs would be comparatively slight, and there would be no serious difficulty in the way of extensive structural improvements; but, in large cities, the formation of new thoroughfares and the rebuilding of neglected districts ought in all cases to be accomplished with much caution and by degrees. It has been proposed to prohibit entirely the use of cellars as places of abode, and at once to eject their tenants. In Liverpool, this was enjoined by an act, doubtless with the most benevolent intentions; but so little had those who framed it considered its probable results, that they had made no provision in the way of new places of residence for the large population which would have been left without houses had it been actually carried into effect. Nor must it be forgotten that the cellars of Manchester and Liverpool are no worse, in a sanatory point of view, than an immense number of ordinary dwellings, and there appears to be no good reason why their extinction should not, like the remodelling of the latter, be effected by degrees. At any rate, it is only reasonable that such of them as are provided with a fire-place and window, and whose foundations are dry, should be allowed to remain, and that the disuse of the rest as places of residence should not be insisted upon, until adequate and available accommodation has been secured for their inhabitants elsewhere.

Much might in the meantime be done to promote the external ventilation of courts and alleys, by removing such walls or houses as close them up at either end so as to prevent a free current of air.

Building.—One of the most important principles to be borne in mind in the construction of dwellings for the poorer classes, is that they should be reared at as small an outlay as is consistent with a due regard to sanatory requirements. Should this rule be disregarded, the rents will of course be high, and overcrowding will be the consequence. Accordingly, those who are best able to judge of such matters are of opinion, that it would be unjustifiable to add to the expense of such tenements by insisting, as has been proposed, on increased thickness of their walls, on the ground that moderate thickness is sufficient for stability; and that party walls, to prevent the extension of fires, are equally uncalled for, as fires are of extremely rare occurrence in that class of houses, a circumstance principally attributable to their seldom being left unoccupied. The sum saved to each house by dispensing with party walls would generally be sufficient to defray the cost of furnishing it with all the essentials of efficient drainage, and of introducing water. It is at the same time an ascertained fact, that houses may be provided for the working classes, furnished with every requisite of comfortable and salubrious habitations, at the same or even at a less rent than they pay for their present accommodation,—that the builders will reap a fair return for the capital so expended, and this without taking into account the saving that would be effected by dispensing with party walls.

It is indispensably requisite, in a sanatory point of view, that all houses to be built in future should be provided with a water-closet, house-drain, water-pipe and sink; that the number of their windows should be proportioned to their cubic contents; that the windows should be of certain dimensions, and should open from above; and that, in the event of their being built without sunk stories, their foundations should be sufficiently deep to ensure dryness. The rooms should be small, as the inevitable consequence of their being too large, would be to increase the rents and hold forth a strong temptation to overcrowding.

The best rule that can be laid down as to the width of courts, is that contained in the twenty-third recommendation of the Health of Towns' Commissioners, and which leaves no room for evasion.*

With regard to the style of building most appropriate for the residences of the working classes, there are various reasons for preferring the system of building in separate flats, as practised on the continent and in Scotland, to that prevalent in England. One great advantage of the former plan, is the much finer and more imposing appearance which a town built in that manner presents, than one consisting of an interminable stretch of small two-storyed houses. The flat system also appears to be more favourable to the existence of that independence of each other so desirable among the poor, and which must suffer to some extent where a number of families occupy the same house, and are only separated from each other by thin partitions, as is frequently the case in English towns. It is true that there may be subdivision of *flatted* as well as of *self-contained* houses; but this, besides not being so common, might be to a great extent prevented for the future, by reducing the size of the separate and independent compartments of which each flat consists; whereas any attempt to reduce the size of self-contained houses, so as to suit the poorest class of tenants, would call into existence a number of streets even more insignificant and paltry in their appearance than the worst specimens of that class now existing. Nor is there anything in the flat system essentially incompatible with a high sanatory state, although the bills of mortality of some of our large Scotch towns are unfortunately too well calculated to induce such a belief. The most cursory examination is sufficient to show that the unhealthiness of such towns is attributable to the total neglect of scientific principles, observable in their construction, as well as to the absence of drainage and the want of an adequate supply of water, and not to the circumstance that the houses are built in flats. In some parts of the old town of Edinburgh, for instance, the rule that the width of streets should increase with their height, appears to be reversed; and houses eight or nine stories high are separated by lanes as many feet wide. Besides the extremely defective external ventilation which such a state of things implies, houses in these situations are quite destitute of house-drainage, and are only supplied with water by public wells. There is no reason why the flat system should not be attended by as high a sanatory state as any other mode of building, provided it be carried out in accordance with sanatory requirements, *i. e.* that the streets be of a width proportioned to their height, which should not exceed three, or at most four stories, and that each flat be supplied with water and means of drainage.

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* "Courts and alleys should not be built of a less width than twenty feet, and should have an opening of not less than ten feet from the ground upwards at each end; the width of the court being in proportion to the height of the houses."

Constitution and duties of administrative bodies;—most advisable mode of defraying expenditure of improvements, &c.—Two great barriers to sanatory improvement have been the number of independent and often conflicting jurisdictions into whose hands its various subdivisions have been entrusted, and the defective constitution of local administrative boards, which in too many instances have been swayed by local interests, and whose proceedings are frequently characterised by such a want of intelligence and uniformity as might be expected from the circumstances, that no qualification as to scientific attainments is generally exacted from those who are chosen to serve on them, and that the latter are generally elected for a limited period. To obviate these evils, it has been proposed by the Health of Towns' Commissioners that all the departments of sanatory improvement should be placed under the control of a local administrative body, to be appointed in every town, and to be subject to the supervision of the crown. With the same view, Lord Lincoln's bill contains clauses which seem worthy of adoption, providing that these boards should be elected partly by the proprietors and occupiers of houses of the rateable value of L.10, partly by the trustees of the local acts, and partly by the town-council, and that one-third of their number should go out of office every third year.

In the event of such a board being formed in every town and district, the powers with which it appears desirable that it should be invested are the following.

1. To construct sewers wherever they are wanting and enforce universal house-drainage, by means of duly qualified officers appointed by itself,—on one general plan,—and otherwise in conformity with the principles formerly laid down.

It would be found difficult, and in many cases impossible from the want of space in the confined houses of the poor, to introduce a water-closet into every house. The arrangement to be adopted with regard to this very important point would consequently depend on the circumstances of different towns and districts with respect to accommodation, and it would probably be sufficient that the local administrative body should be directed to make their introduction as general as possible. In certain cases, the establishment of well-regulated public water-closets might compensate for the want of sufficient provision of this nature in private houses, and in Scotch towns, a water-closet would be sufficient for every flat.

2. To enforce the general introduction of water into private dwellings, and to procure a sufficient supply for this and the other purposes formerly mentioned. The propriety of introducing water into the houses of the poor, in preference to the plan of supplying them by means of public wells, is universally acknowledged. There are many objections to that expedient, the principal of which is the labour thereby entailed upon the poor of conveying the water from such wells to their houses,—which labour, if properly applied, would in most cases be sufficient to produce a return by far exceeding the cost of a constant supply of water in their own premises. Another great evil is the natural effect of such a state of things in causing an undue economy of water among the poor.

The selection of the mode of supply, whether on the intermittent plan or that of constant supply at high pressure, must depend on the physical advantages of different towns, the latter being adopted in all cases where it is practicable.

The imperfect supply of water afforded by public companies has been a fertile source of complaint in almost all large towns. The only effectual check which the public has upon water-companies, is that afforded by the formation of rival establishments, an expedient which may effect the desired object in the meantime, but sooner or later leads to an aggravation of the evils complained of. The experience of London and other large towns shows that the reduction of price which in such cases ensues at first, soon becoming ruinous to both parties, leads eventually to an amalgamation and a renewal of the monopoly. Even were the companies after their union willing to afford water at as low a remunerative rate as possible, it would be impracticable for them to do so nearly as cheaply as might have been done before the second company was formed, as the same revenue has now to pay interest on two capitals, and defray the expenses of two separate establishments, as well as the wear and tear of two sets of pipes, as formerly had only to pay interest on one capital, and defray the expenses of one establishment. Competition thus being the only species of control which the public are capable of exerting over water-companies, and its ordinary results being such as have been stated, it has frequently been proposed to secure the safety and interests of both parties, by placing the latter under a public trust. On this ground, it appears advisable, in order to facilitate the supply of towns with water under the proposed sanatory arrangements, that in all cases where the companies are willing to agree to such a proposal, the management of the water-works be transferred to the local administrative body, on such terms as the company will accept.

When no such arrangement can be effected, the recommendation of the commissioners may be adopted, which is that, "where any independent body has the management of the supply of water, it be liable to comply with the demand of the local administrative body on equitable terms." The commissioners also recommend, "that on the establishment of new companies, it be made a condition that the local administrative body be enabled to purchase the works after the lapse of a certain number of years, upon certain terms, and upon a rate of interest to be fixed." There seems, however, to be no good reason why the local administrative body should not themselves be empowered to construct whatever additional works may be required.

3. To take measures for the gradual improvement of crowded districts, by opening new thoroughfares and rebuilding and widening streets and courts, and to raise money for this purpose. For reasons already assigned, this must be done with the greatest caution,—care being taken that no district be so improved until available accommodation shall have been provided for its inhabitants elsewhere. No time should, however, be lost in removing such walls or buildings as obstruct the ventilation of courts.

4. To regulate the width of new streets, courts, and alleys, and to see that all houses be constructed in conformity with the principles formerly laid down.

5. To raise money for building streets in the suburbs, or the nearest vacant spaces, for the accommodation of those who may, from time to time, be ejected from cellars, or driven, in the progress of structural improvements, from crowded districts.

6. To provide for the gradual abandonment of cellars as places of residence, excepting such as possess the qualifications for exemption contained in the 24th recommendation of the commissioners.

Lord Lincoln's bill contains a clause to the effect, that all cellars, with certain exceptions, should cease to be occupied immediately after the act should come into operation. But supposing that such a measure were carried into effect, and even that places of residence had been provided for their inmates in the suburbs, the difficulties formerly stated in connection with the accommodation of the ejected inhabitants of improved districts would present themselves. Instead, therefore, of laying down any regulation as to the precise period when residence in cellars should be prohibited, the safer plan would be, to leave the matter in the hands of the local administrative body, who might proceed to effect the object of closing them in detail, and would consequently soon be in possession of accurate information as to the most advisable mode of doing so.

7. To cause farther inquiries to be made as to the cheapest and best means of promoting ventilation,—to enforce it, when they shall deem it expedient to do so, upon places of public resort and assemblage, and more particularly upon schools, workshops, and manufactories,—to see that all public buildings, and private houses yet to be built, shall be provided with such structural arrangements as shall, after due investigation, be considered necessary for effectual ventilation,—and by diffusing popular information on the subject, to endeavour to effect the general introduction into the houses of the poor, of such a simple apparatus for ventilation as experience shall have shown to be most economical and efficient.

8. To superintend by its own officers the paving and levelling of streets and courts.

9. To provide for the daily cleansing of all streets, courts, and alleys,—for the removal of all accumulations of dung and refuse, and for the cleansing of cesspools, until they be superseded by properly constructed drains, and to appropriate all such refuse.

10. On the occurrence of any epidemic disorder, to require the landlords of all property in the affected districts to whitewash and fumigate their houses; and in the event of their delaying to do so, to cause it to be done, and to recover the expense from them. This may be most cheaply and effectually accomplished according to the plan adopted and practised with the best results by Mr Ramsay, late superintendent of cleaning in Edinburgh, to whom, I believe, the credit is due of having introduced the practice of house cleansing on a large scale.*

The appointment of a medical officer in every district by the local administrative body, to investigate its sanatory condition,—to ascertain the existence and causes of disease,†—to point out the best means of arresting the spread of epidemics, and to superintend the ventilation of public buildings and lodging-houses, has been recommended by the commissioners, and would undoubtedly have the effect of extending our knowledge of the causes of disease, and of promoting the public health. They also recommend very strongly that all common lodging-houses should be licensed, and conducted under such regulations as shall appear best calculated to promote the health of their inmates. They might be advantageously subjected to the surveillance of the medical officer of the district.

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* First Report, vol. ii. p. 383.

† A proper system of registration is essential to the right performance of these duties. It is extremely desirable that the registration act should be extended to Scotland without farther delay.

In conclusion, no measure for the sanatory improvement of large towns would be complete which did not make provision against the evils arising from interment in towns,—for the substitution of slaughter-houses in the suburbs, similar to those established at Paris, for those now in use, and for the abatement of such nuisances as the smoke of steam-engines, and the noxious exhalations of factories.

One great obstacle to the extension of drainage and the more general introduction of water into houses, is undoubtedly the unwillingness of proprietors, who may have only a short interest in the property to be improved, to defray the entire expense of what is to be a source of permanent benefit. The witnesses who were interrogated by the commissioners on this subject were unanimously of opinion, that the best mode of obviating this difficulty would be to spread the charge of such improvements as a rent over a series of years. Accordingly, the commissioners have recommended that the local administrative body be empowered to borrow, on the security of the rates, such sums as may be requisite for the execution of works of drainage, and "that the expense remain a charge upon the properties, to be levied by a special rate upon their occupiers, and recovered with interest by annual instalments within a certain number of years, unless the owners prefer to pay the cost in the first instance," or the improved property be below a certain rental, when the landlords are to be rated, (the amount to be added by them to the rent,) "such a deduction being made as may be considered a fair equivalent for the labour and losses incident to the collection of rents on such property." It has been calculated that the whole expenses of drainage and the introduction of water, if spread over a period of 30 years, would not amount, with the addition of 1d. a-week for a supply of water, to more than 2½d. per week; but supposing that this sum were doubled, the annual outlay thus entailed upon the working man would be far more than counterbalanced by the saving which would be effected in diminished liability to disease. The amelioration which would eventually ensue in his circumstances in other respects, would, as formerly shown, make such an outlay a source of great ultimate gain; and the willingness which this class has in various instances exhibited to pay an increased rent for sanatory improvements, shows that they are fast becoming alive to a sense of their importance, and would hail with satisfaction any measure for their general introduction which could be accomplished at a reasonable cost to themselves. There is, however, in all large towns, a class so poor, as to be barely if at all able to pay for the hovels they reside in, a weekly rent, in some cases, not more than double the probable weekly rent entailed by sanatory improvements, and who, at present at least, would be quite unable to bear the smallest addition to it, although there is no doubt that, in the course of years, their circumstances would be so much improved as that they could easily pay more than double the sum for domestic accommodation which they now do, for being the class which suffers most severely from a neglect of sanatory laws, they would derive the greatest proportionate benefit from an amended state of things. It is even questionable if it would be possible to succeed in borrowing the money requisite to effect the desired improvements on the security of rates levied upon such a class of tenants. It has too often been found that the exemption of tenants from any share in the payment of expenses incurred in improvements made in property of this class, has resulted in the gain to the landlord of the amount of the exemption, which he has added to the rent. No such result, however, could follow the improvement of the class of houses in question at the public expense, as the landlords of such property, having the greatest difficulty in collecting the rents as they stand at present, would at once see the absurdity of any attempt to raise them. The great revenue derivable from the proper application of the sewer water of towns was formerly alluded to, and it would be difficult to find a more legitimate outlet for a portion of it than would be furnished in its appropriation to the object in question. Such a revenue would clearly be public property, and the improvement of the worst districts of large towns, although more immediately benefiting one portion of the community, would in the end be productive of the greatest advantage and economy to the whole. It would be impossible to lay down any general rule as to the rental at and below which houses should be improved in this manner. This would depend in a great measure on the value of property in the different towns to be improved.

A considerable proportion of the class of people referred to are dependent on the parish for support, and as the parishes would ultimately be relieved of a large amount of the burden imposed upon them by the excessive and in great degree preventible mortality consequent on a low sanatory state, it would be reasonable that they should defray the expenses of sanatory measures so far as incurred by those receiving aid at their hands.

In conclusion, I have only to express a confident hope that this inquiry will immediately be extended to Scotland and Ireland, where it is not less urgently necessary than it was in England, and that government will, with as little delay as possible, bring forward such a measure, as the exigencies of the case demand.

ART. VIII.—*Termination of the case of Mary Riordan, published in Vols. IV. and V. of Transactions of College of Physicians in Ireland.* By WILLIAM PICKELLS, A. B., M. D., one of the Physicians to the Cork Fever Hospital.

To the Editor of the Edinburgh Medical and Surgical Journal.

SIR,—A medical friend having lately directed my attention to a paragraph in an article in the last number of the Quarterly Dublin Medical Journal, containing the review of a work on epidemic insanity, in which a remarkable case published by me many years since, in the Transactions of the College of Physicians in Ireland, "of a young woman who continued to discharge during several years insects of different sorts, and in different stages of their existence," is, in the most unqualified manner, treated as a delusion. I beg to say that the writer has been altogether misinformed; that the facts of the case are but too true; that many persons of respectability, besides myself, saw her repeatedly discharge, under the operation of emetics, larvæ of the species of beetle, denominated *Blaps mortisaga,* from the stomach, (the channel by which they were for a long time usually voided; not, as implied in the article alluded to, exclusively *per anum,*) and that on reference to the history of the case, the writer would have seen no ground for classing it with those fraudulent and unblushing impostures which have been from time to time palmed on the public credulity. In addition to other vouchers of its authenticity, mentioned in the history of the case, the present mayor of this city, A. Roche, Esq., who is himself a member of the medical profession, has authorised me to say, that he sent to a friend in Edinburgh several larvæ of *Blaps mortisaga*, at the time she was discharging them, which had been vomited under the operation of an emetic in presence of a number of persons, (as occurred more than once,) in the public shop of the dispensary of this city.

The patient has since died; having during the last twelve years of her life been confined to her bed by paraplegia. For several years after the extirpation of the insects by, as stated in the history of the case, six ounce doses of the oil of turpentine, she continued, with the exception of the occasional return of convulsions, to enjoy tolerable health, her other distressing complaints, vomiting of blood, retention of urine, dropsical swellings, &c., having all disappeared. During this period she married a person named Duggan, a hatter by trade, who died in two or three years after in hospital in Dublin, whither he had gone, accompanied by her, to seek employment. The last time I saw her was in July 1830, upon her return from Dublin, after the death of her husband. She was fat and ruddy, and appeared to be even strong. She then informed me of the particulars which have been mentioned, and in addition, that she had had two children, of whom one was still-born, the other, a seven month's child, who died in convulsions three hours after birth. In the course of the same year, or in the following, she became an inmate of the workhouse of this city, in which establishment she died. While in the workhouse, Dr Edward Townsend, the physician of the establishment, supposing the convulsions were still kept up by a remnant of the insects, gave her, as he informed me, repeated doses of the oil of turpentine to test the cure, but there was no return of the discharge of insects. The convulsions continued occasionally to recur to the last, exhibiting from the irritability of the cerebro-spinal system, as sometimes happens in the case of epileptics, the melancholy spectacle of one-half of the body being convulsed, while the other half was paralytic. Dr Townsend has authorised me to say, that during the period of his long attendance on her, which was of several years, nothing transpired which could have led him to suppose that there had been ever any attempt at imposture. She died at an age of over 40. *Post mortem* examination would not be permitted by her friends. The prejudices of the humbler classes here are, it is well known, strong against it.

As I was in the habit of minuting the observations as the facts occurred, it may perhaps not be amiss, nor on the present occasion uncalled for, to give from my journal the particulars of what occurred, in the first instance, of my having witnessed the discharge of insects. "After having been about ten minutes or upwards in the room, the emetic which I had seen given to her myself began to act. She threw up the liquid (water) she had taken with it, and *seven* larvæ of *Blaps mortisaga.* She was at this time sitting on a chair. She now swooned, and fell on the floor, becoming stiff and motionless, her arms lying inflexibly by her side, and her hands clenched. The knees continued bent; her aunt, who was generally her care taker, and who was present, remarking that whatever posture she was in at the time, the fit came on, in that she continued fixed. After some time she began to work in convulsions, the violent convulsive jerking of the recto-abdominal muscles was particularly remarkable, as was also the gurgling noise (borborygmi) of her bowels, as if water had been poured into an empty cask, and you were rinsing it. This noise, sometimes so great, her aunt says, as to be heard below stairs. When the convulsions are over, the state of rigidity and insensibility usually returns, and continues for some time, its termination being known by her hands falling and her limbs relaxing. Upon recovery from the fit, she was placed on the side of the bed, and vomited immediately after more of the liquid taken in, intermixed with green bile. On pressure, the abdomen felt tumid and hard as a board. She soon fell off the side of bed and got into another fit, her eyes staring full as if at the observer. Upon recovery from this fit, she writhed, screamed, snorted, then yawned and stretched, as if waking from sleep; complained of cold, and wished that the fire would be made better, though the day was warm; during a fit, her aunt observing, one side of her will be cold and the opposite side warm. Complained of a 'tearing' sensation 'of her bowels.' Had not the slightest recollection of what occurred previous to the invasion of the paroxysms. Inquired whether she had taken the medicine, and she seemed quite surprised at my presence, though I had been talking to her before the access of the paroxysms. She had, it appeared to me, three fits, including one while she was on the bed. When she discharges her stomach freely, the jerking of the bowels, as her friends term the spasmodic action or subsultus, soon ceases, proving that it is the insects which occasion it. When she lingers this way a long time without discharging her stomach perfectly, the jerking is often repeated. The fit, or rather repetition of fits, sometimes continues for hours. A month since she had a fit, which continued from evening until morning."

I remained in the room upwards of two hours. On the following day I learned she had had another fit after I had left.

In one instance, during the tossings of a convulsive paroxysm, a large metal hair pin, which had been incautiously allowed to occupy the place of a comb in her hair, penetrated the integuments, adding much to her sufferings before it was extracted.

Besides the convulsive paroxysms described, she was liable, I learned, to attacks of catalepsy, or a disease resembling it, the duration of the paroxysms varying from some minutes to hours.

If seized while taking a cup of tea, the cup was arrested in its progress, and held grasped firmly. If while sewing, the operation of the needle was suspended in like manner. If while washing, the hands were arrested in the act of rubbing, one over the other, and the article grasped firmly. She was liable to be seized quite suddenly, as while standing in the middle of the floor, or even while talking to another person. I will not further retrace the harrowing narrative of her sufferings during the long period previous to, and during the discharge of insects, than to add, that they were at one time so intense, as to goad her into occasional paroxysms of mania, the approach of which was commonly indicated about a quarter of an hour beforehand, by her eyes becoming, as the aunt expressed it, " still as dolls." During the paroxysms, which sometimes lasted for hours, several persons could with difficulty restrain her.

Though the other beetle larvæ, and the dipterous larvæ, and the ascarides, discharged from time to time, as specified in the history of the case, contributed, doubtless, their share of irritation, the larvæ of the *Blaps mortisaga*, or *churchyard beetle*, were evidently the principal tormentors, the number of which she continued to discharge for three years and nearly three months, that is, until they were killed off by the enormous doses stated, of the oil of turpentine, may be said to be astounding. The average number discharged in each successive instance not diminishing, but, as appears from the registry, rather inceasing, to the last, as if they continually repullulated, or, to use the patient's expression, " spawned." Until under the operation of large doses of the oil of turpentine, they past down *per anum*, they were all, with few exceptions, extruded from the stomach, the discharge being commonly attended with vomiting of blood, and in almost every instance with convulsions. Many which I measured were an inch and half in length, and four lines and half in girth. I gave several to the itinerant proprietor of an oxyhydrogen microscope, who visited this city, and afterwards other towns of the empire. Though sufficiently evident, without the aid of the microscope, the terrible conformation of their jaws and feet was made fully apparent by means of this instrument. The forefeet in particular were tremendous.

I am aware that it is a point much disputed, whether intestinal worms pierce the coats of the intestines, or any living texture. The question may arise, in regard to intestinal worms, strictly so called, which are the natural inmates of the human body, but it can hardly, I think, be raised in the present instance. Under a persuasion, from considering the powers of annoyance with which they were armed,coupled with the symptoms of the case, that these animals penetrated the living texture, anticipating during life the work of death, I could not help, in witnessing her real sufferings, sometimes recalling the description by the most philosophical of poets,—of the fabled punishment of the vulture perforating the vitals of Tityus.

In the Cyclopædia of Practical Medicine, article Worms, a case is cited, published by Ariel, in the Scientific Transactions of Upsal, which, it will be perceived, bears considerable analogy to the present. " An immense number of insects, (all, it appears, beetles of various sorts,) existed in the stomach for the long period of two years. They were at length, after a great number of unsuccessful experiments, expelled by a mixture of turpentine and linseed oil. Of various species of staphylinus,* there were 263 in all. The larvæ were almost past counting, especially those of the *Tenebrio molitor* and the *carabus.*"

Larvæ of insects are, there is reason to think, much oftener eliminated from the stomach and intestines, both by children and adults, than persons in general appear to be aware. The writer of the article entomology in Rees, goes the length of saying, "that certain kinds of these supposed worms, which infest the human race to the destruction of thousands, in the age of infancy, are really no other than the larvæ of insects." The present mayor of this city sent me, in January 1830, a larva, which was proved to have been voided *per anum* the evening before, by a child, a male, of only three months old. It was alive. The evening before that on which it was voided, the child's mother thought it would have died from the violence of screaming and convulsions. The child, though healthy-looking, as I saw, could bear scarcely any thing on his stomach since birth. The larva I ascertained, by the peculiar termination of the tail, to be that of the *Blaps mortisaga.* From frequent observation, I may add, the larva of this insect is what naturalists term "amphisbænous,"—being enabled, by means of a foot in the tail, to walk with facility backwards as well as forwards.

In an early volume of the Philosophical Transactions a case is given by Mr Jessop, of a girl of eight months old, who vomited three hexapodes, (larvæ, it would appear, of beetles,) all very active and nimble.

The following, copied from the Cumberland Packet, is taken from the Dublin Weekly Register, July 26, 1823. "A woman named Graham, about sixty, of Harrington, Cumberland, has, during more than twelve months, been afflicted with pains in the head. Lately, a considerable degree of rigidity took place in the muscles of the lower jaw, and continued nearly five weeks, when the patient, who is in the habit of taking snuff, having occasion to apply her handkerchief to her nose, from which, by expelling the air with considerable force, she discovered that she had voided a worm of about the size and appearance of a caterpillar. Its head was black, with two feelers; it had a great many feet. The patient, since discharging the worm, has continued perfectly free from headach. It is conjectured that the worm must have been lodged in one of the sinusus of the frontal bone, communicating with the nose."

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* To the staphylinus kind belongs the species of beetle branded in this country by the vernacular name "Dearg Diabhl," (Red Devil); in England, known by the names of the roving beetle and the devil's coach-horse. In this country it is an object of intense odium and abhorrence from a popular legend connected with it.

Burserius, vol. i. p. 433, alludes to "the fever arising from the rending headach, occasioned by worms in the frontal sinus." A case of larvæ of the œstrus discharged from the maxillary sinus is given in Duncan's Annals of Medicine, vol. vi.

The following, copied from an American paper, is taken from the Cork Southern Reporter, July 29, 1828 :—"*Astonishing fact.* —There died recently in the town of New Stonington, Connecticut, a woman, aged about 40, who had been ill a long time, and complained of excessive pain in her heart. She left a request that the physician who attended her should examine the cause of her extreme suffering. The request was complied with, and in the centre of the heart there was found a *living worm*, an inch and quarter long, and of large size." The so-called worm was not improbably, as the worm so called in the Cumberland case, the larva of an insect; the received nomenclature including under the general name "worms," the larvæ of insects, though there is this marked difference, that worms undergo no changes. Worms have been found in the liver, spleen, &c.

Many instances might be adduced of diseases which had long resisted other treatment, having yielded to anthelmintics. In a paper published some years since in the ninth volume of the London Medico-Chirurgical Transactions, Dr Rumsey gives various cases of lymphatic hæmorrhage and pulmonary affection which had been cured by an anthelmintic course, the lungs, as well as the brain, having been found frequently to participate in the disease, and at times, the uterus; a profuse hæmorrhage having ensued from one or other organ, and been repeated, at uncertain intervals; in some instances without any suspicion of the real cause, and, consequently, with an erroneous practice. His plan was to strengthen the system generally, and then give anthelmintics. The late Surgeon Evans of the Douglas Dispensary, near this city, informed me that he brought off from a patient, the wife of an artilleryman, who had laboured, upwards of eight years, under tic douloureux of the infra-orbital nerve, a tape worm, five yards and a half long, with a mixture of half an ounce of castor oil and half an ounce of oil of turpentine. The bowels had been constipated for several days previously. She continued to be much relieved after the discharge of the worm. She had been treated by medical men in Dublin, London, and other places, without benefit.

The history of the case which forms the principal subject of this paper, independently of any physiological interest it may possess, is, it will be perceived, not without practical utility; illustrating, as it fearfully does, a hitherto much neglected article in the catalogue of human diseases,—that of diseases arising from the larvæ of insects,—neglected, though, as a branch of natural history, the study of the minute part of the creation, under the name of entomology, has of late years attracted great and general attention. Though the case eventually terminated fatally, the efficacy of medicine was proved, by the extermination of the insects, and, with the exception of the occasional return of convulsions, the enjoyment of at least tolerably good health for several years after;—the convulsions, which had become established from long continuance of years, complicated with that worst of all diseases, extreme poverty, having ultimately terminated in paraplegia. The inefficacy of mere antispasmodics was proved, by doses of æther having been given to a great extent, and during a long period previous to the employment of means for the expulsion of the insects, without any other beneficial effect than that of relieving "the cramp and pain of the bowels,"—an effect, however, which it appeared to have lost by continued use. Though partaking much of the character of hysteria, the convulsive affection under which she so long laboured, appears, on the whole, to be nosologically referable to epilepsy. Among other symptoms marking great sensorial disturbance, I might have mentioned that, at one time, in reading, as she told me, the letters appeared green, and the lines confused, each appearing higher or lower than its true situation,—the former symptom indicating a degree of glaucoma, the latter exemplifying, it would seem, what is technically called "interrupted vision (*visus interruptus*)."

In my first paper, in the fourth volume of the Transactions, mention is made of a mode somewhat novel, which was accidentally discovered, but which proved efficacious in several instances in which it was resorted to, in cutting short the convulsions, the mode by unclenching the hands, &c. having been often tried and failed. I used the expression, somewhat novel, because I had heard, that in the West Indies, by a mode not very dissimilar, that of introducing a straw or reed into the urethra, they sometimes succeed in stopping tetanus. In Liston's Lectures on Traumatic Tetanus, Lancet, January 1835, may be found full particulars, as detailed from Mariner, of an analogous, but, if the general spasm be very violent, much more severe practice in the cure of tetanus, by the natives of the Tonga islands.

The discharge of insects in the more advanced stages was confined almost exclusively to the autumn of 1822, the year of the commencement of the expulsion of insects. The number of instances was extremely rare, compared to that of larvæ. The term by which she distinguished the insects in the more advanced stages was that of "winged ones," under which she included both perfect insects, or imagos, and pupæ, the elytra or wing-cases having given rise to the common appellation. The total number of such ejected was estimated at about sixty. From having been often found turned on their backs, probably in the pupa state, they were compared by her family "to lobsters." Their discharge was attended with great, much greater than in the case of the larvæ, sense of distension about the pit of the stomach, or as she expressed it, "bloating." For a considerable portion of the period during which she was discharging them, I regret I was absent in Spain, otherwise I have no doubt that many more specimens of this description would have been preserved for examination. The number obtained was five pupæ and two imagos, which it was proved were deduced from the beetle larva which was voided in the greatest numbers, namely, that of *Blaps mortisaga*, thus establishing the interesting fact, that the different successive metamorphoses of insects may take place in the human stomach and intestines during life. During the latter end of the summer, and during the autumn of 1824, I had frequent opportunities of seeing the beetle larvæ in the act of casting the cuticle. In one instance I saw it cast off entire, from the head downwards, like a sheath, in the manner of serpents. Two immediately after having shed the cuticle were devoured by their fellows. In the experiments connected with the case, there was great difficulty in rearing the larvæ of blaps, owing to their proneness to devour each other. The process of shedding the cuticle, with the spectacle of which the patient was familiar, but the meaning of which she could not understand, she called "throwing off their jackets." As illustrative of insect vitality, or at least of the tenacity of life of blaps, I may mention, that in two instances I saw portions of mere cuticle, with the feet attached, move about actively on the feet.

Of the dipterous kind, with the exception of a few, which were fast passing, or had passed into the pupa stage, when discharged, no specimens in the more advanced stages were obtained, though Dr Thomson, who has since become so distinguished by his "Zoological Researches," particularly by his discoveries in Australia, and myself succeeded in hatching a number from the larvæ. Of the perfect insects thus obtained, one species appeared to be that of the *musca larvarum*, Linn., a species remarkable for depositing its ova, as insects sometimes do, in the living larvæ of other insects. Kirby extracted a live insect from the abdomen of a wasp. The connection of the autumnal season with the multiplication and development of the insects was striking. Of one thousand larvæ of blaps registered by me as having been submitted to my observation, in the year from February 1, 1824, to February 1, 1825, 648 were discharged during the autumnal season. Hippocrates in his book "on air, water and situations," has the following passage, which is not inapposite: "The autumn is most productive of living creatures in the bowels. The thin worms called ascarides are most troublesome in the evening, resembling the autumn, the time of the day they generally give most uneasiness, not only from an increase of pain, but also from their own proper nature." The autumn of Hippocrates commenced with the rising of Arcturus and ended with the setting of the Pleiades, which, as Dr Maskelyne has calculated, would be about the 26th of October. The evening exacerbation in continued fever, not merely in that arising from worms, but in continued fever in general, is a phenomenon which has been long observed, but which has not, I believe, been well accounted for. Galen somewhere directs that when bleeding is resorted to in fever, the operation should be performed in the evening.

The ascarides alluded to by Hippocrates are probably the small white acuminated worms, so called, which it is often found so difficult to dislodge from their sacculi in the intestines, though Heberden says, "they sometimes creep out of the bowels of themselves, and sometimes, as I have been told, through the nostrils." The ascarides, of which specimens were preserved, as having been voided in the present case, were generally of considerable length. The greater part, thread like and tortuous, resembled most those which are found in the stomach of the cat (*Ascarides felis*). The rest were of the species of the *Ascaris lumbricoides*, (the common lumbricus). Instances occurred of their having been voided in winter. I have known instances of the common lumbricus being voided during the same season in fever. I mention this, because while one naturalist (Dr Fleming in his Zoology) states, "Of the hybernation of intestinal worms nothing is known;" another says, "Intestinal worms, like all cold blooded animals, bear cold well, but an increase of heat badly." Of larvæ of blaps, the number discharged during the three winter months, from November 1st 1824 to February 1st 1825, was sixty-seven.

In the present paper, additional evidence has been afforded of the authenticity of the case of Mary Riordan. Even without the addition of this evidence, the particulars, as published in the Transactions, appear to have gained, from those who read them, general credence. The writer of the article above mentioned in the Cyclopædia of Practical Medicine, after citing the case which occurred in Sweden, prefaces an abstract of the case, which occurred in this city, with the words, "Of all the marvellous cases of this sort, on record, one of the most so, and yet at the same time one of the best authenticated, is that by Dr Pickells, in the 4th and 5th volumes of the Transactions of the King's and Queen's College of Physicians in Ireland." The writer of the article, "Insect Transformations," in the Library of Entertaining Knowledge, prefaces his account with the observation, "That insects are in some rare cases introduced into the human stomach has been more than once proved, though the greater number of the accounts of such facts in medical books are too inaccurate to be trusted. But one extraordinary case has been completely authenticated both by medical men and competent naturalists, and is published in the Dublin Transactions by Dr Pickells of Cork." Mr Rhind in his work on intestinal worms has also given an abstract of the case without questioning its authenticity. Considering the many monstrous and barefaced delusions, which have been, from time to time, practised on the public credulity, it was not to be expected that a case so anomalous and heteroclite, in many respects, would on a first view be received without considerable suspicion and distrust.

Scepticism, in regard to the credibility of facts, which deviate from the ordinary course of things, useful by subjecting them to a severe and cautious criticism, may, however, if carried beyond a certain extent, become as dangerous in physical science as credulity. "We every day," observes an eminent writer, "see more reason to extend our *à priori* formed laws of nature. Our knowledge in many branches of natural history would be much less imperfect than it is, if many facts had not been suppressed, either from a fear lest they should be thought incredible, or from that unreasonable incredulity, which will not, even upon the strongest testimony, give credence to any thing it cannot explain."

To the unreasonable incredulity here animadverted on we do not of course address ourselves. Vain would be the endeavour to remove the film from its eyes, for in the expressive words of Galen, "there are some persons of so refractory a disposition, that even if Apollo or Æsculapius wished to instruct, or to advise them, they would not attend. Wherefore to them I write not."

The horror and disgust felt towards the beetle insect in this and other countries of Europe, is a remarkable instance of the effects of prejudice, arising probably from its being associated in our minds, owing to the repulsive situations in which it is usually found, with the ideas of filthiness and darkness. Yet among the exotics of this tribe are to be met with some of the most brilliant objects in nature. The diamond beetle, for the brilliancy and richness of its colours, is not perhaps exceeded in the whole insect race. The phosphorescent insect, the glow-worm (*Lampyris noctiluca*) of the south of Europe, and other hot climates, bespangling and embellishing in so picturesque a manner, particularly in tropical climates, the horizon at night, is a species of beetle.

Mr Baker, in an early volume of the Philosophical Transactions, endeavours to prove that the sacred beetle of the ancient Egyptians was the *Blaps mortisaga*, which has been so often mentioned in this paper, its tenacity of life, so wonderfully exemplified in his experiments by its having lived three years without food, rendering it, in his opinion, a "fit symbol of the Deity or of Eternity, as it is probable," he adds, "the onion was held sacred by them from its representing the orbits of the planets." A different interpretation has, however, been given of the worship of the beetle by the ancient Egyptians. Pomet in his history of drugs, gives, under the article "Embalming," a history of the beetle from Kircher, and of the cabalistical mythology of the ancient Egyptians in regard to it. "The principal of all those animals," the account says, "or that for which the Egyptians had most veneration, was the beetle, as well because of its wonderful birth, or production, as from the analogy or resemblance they pretend the animal has with the sun, and the strange instinct in it to continue its species. They are of several kinds, but those for which the Egyptians have the greatest veneration, are those which have a head like a cat, accompanied with rays, which give occasion to them to believe that these animals have some analogy to the sun, and the more because it has thirty little paws like fingers, which represent the thirty days that the sun makes each month in passing through the signs of the zodiac." The account then goes on, according to the hypothesis of equivocal generation,—a doctrine which has been revived in our time by some eminent naturalists, both in Germany and in France, to describe the mode in which this little animal continues its species without the aid of the female, "seeking out the dung of an ox, or bull, and having found it, forming a round ball of the figure of the world, which, with his hind feet, he turns from east to west, and turning himself towards the west he imitates the motions of the world. Having thus *rolled the ball*, he puts it in the ground, and leaves it there twenty-eight days, which is the time that the moon passes through the signs of the zodiac; and during that time he hatches the little beetles in the ball, and the twenty-ninth day, which is the conjunction of the moon with the sun," &c.

Enough has been said to show that the sacred beetle of the Egyptians was not the *Blaps mortisuga*, as supposed by Mr Baker, but the *Scarabæus pilularius*, or *volvens* of modern travellers. Moore in his tale of the Epicurean, in a note on the Theban beetle, says, "the early Christians used to apply this emblem to Christ." Quoting Saint Augustine, he adds: "Bonus ille scarabæus meus, non eâ tantùm de causâ, quod unigenitus, quòd ipsemet sui auctor mortalium speciem induerit, sed quòd in hâc nostrâ fæce, sese volutaverit, et ex hâc ipsa nasci voluerit."

ART. IX.—*Fatal Case of Military Flogging at Hounslow.*

The Inquest.—Thursday evening, at eight o'clock, a highly respectable jury of thirteen inhabitants of the parish of Hooton were empanelled before Mr Wakley, M. P., in one of the parlours of the George the Fourth Inn, Hounslow Heath, to inquire into the circumstances attendant upon the death of Frederick White, aged 27 years, a private of the 7th Royal Hussars, whose death, it was alleged, had been caused by the effects of the corporal punishment he had received under an order of court-martial.

The room was crowded to excess by the respectable inhabitants of the neighbourhood, and the officers of the regiment, anxious to witness the proceedings.

Thirteen jurors having been sworn, and Mr J. Bond appointed foreman, the coroner accompanied them to the barracks, which was about half-a-mile distant, to view the body of the unfortunate deceased. It was in the coffin, and on being turned round on its face, the back, from the nape of the neck down to the loins, presented a deep purple appearance. In the middle of the back, between the shoulders, where the greatest inflammation had evidently been, a great piece of skin, nine inches one way and eight inches the other, had been cut away.

The coroner inquired where the skin was, and by whom it had been cut off, and it was replied it had been cut off by Dr Reid, who had been sent down by the Horse Guards to perform the *post mortem* examination of the body, and it should be looked for. Much delay was occasioned by the search, and eventually a piece of skin was brought, which, however, was not a fifth part of the whole.

After an absence of nearly an hour, the coroner and jury returned to the inquest room, and their names having been called over,

The coroner inquired if any of the relatives of the deceased were present in the room, or had been informed of the inquiry?

The adjutant of the regiment replied that they were not, neither had he informed them on the subject.

The coroner inquired if any one had written to them?

The adjutant said he believed not, as they only knew from the deceased's attestation where they were to be found, which was stated to be Camberwell, and not what part.

The coroner then addressed the jury, and said it must be apparent to them, from the number of witnesses that must necessarily be examined, that it would be impossible to finish the inquiry that night, however late they might sit; and, therefore, in his opinion, it would be better at once to adjourn, so that they might have all the witnesses in attendance, and finish at one sitting. By doing so, the relations of the deceased would also have an opportunity to be present, as he doubted not that the publicity which would be given to the inquiry in the newspapers would cause them to be made acquainted with it. It would also be necessary to summon several witnesses from London, viz. Dr Reid and Mr Hall. It was essentially necessary that Dr Reid should be present, as he was the person who had removed the skin from the back of the deceased, not a fifth part of which had been found, and what was found would not fit the back. It was also necessary that a medical man, who was totally unconnected with the army, should immediately see and examine the body, and he would leave it to the jury to determine who that surgeon should be. He would only further say that the surgeon chosen should be a gentleman on whose opinion the jury felt they could place confidence, who was quite unconnected with the case, and who would give an impartial and unbiassed judgment on what he saw. The military authorities might also appoint one, who should have every facility afforded to do the same.

After considerable discussion the jury unanimously fixed on Mr Horatio Day, surgeon of Isleworth, who was appointed.

The coroner then, addressing Dr Warren, surgeon of the regiment, said —Sir, I shall rely upon your rendering my constable and the police inspector every assistance in the summoning of the necessary witnesses at the barracks, in order to show the real cause of death, whatever it may be.

Dr Warren promised to do so.

The coroner then bound the jurors in a recognizance of L.20 each to attend on a day next week to resume the inquiry, and at ten o'clock the investigation was adjourned.

July 21, 1846.—Yesterday morning (20th July) the inquiry, commenced on Wednesday last, into the circumstances attending the death of Frederick John White, aged 27, a private in the 7th Royal Hussars, who was flogged on the 15th of June last, and died in the hospital of Hounslow barracks on Saturday week, the 11th inst., was resumed at the George the Fourth Inn on Hounslow-heath, at half-past 9 o'clock.

The great interest exhibited at the opening of the inquiry on Wednesday last by persons of all classes to be present was far surpassed on the present occasion, and by 9 o'clock the inn was besieged by respectable persons from all parts of the surrounding neighbourhood, none of whom were, however, admitted until after the coroner and jury had assembled.

Mr Wakley, M. P., coroner, accompanied by Mr G. J. Mills, the deputy-coroner, arrived at twenty minutes past 9 o'clock; and the jury having shortly afterwards assembled, and their names having been called on, the public were admitted at 20 minutes to 10 o'clock.

The inquest-room was instantly most inconveniently crowded, which rendered the heat during the inquiry most oppressive. Amongst those present we noticed Mr G. Baillie (chairman of the Brentford magistrates), Sir Alexander Spearman, Bart., Rev. H. S. Trimmer, Mr Henry Pownall, and Mr B. J. Armstrong, county magistrates; Dr Mackinlay, Mr C. Eley, and other of the leading inhabitants of the neighbourhood.

Mr G. Clark, solicitor, of New Brentford, and clerk to the magistrates, attended to watch the case on behalf of the regiment.

The coroner opened the proceedings by inquiring if any of the deceased's relatives were present, and was informed that the deceased's brother was in the room.

Mr White, in answer to questions by the coroner, said, he had not seen the body of the deceased, but he was quite sure, from the inquiries he had made, of its identity. Deceased was 27 years of age, having been born in January 1819.

The coroner then desired that no witness but Mr H. G. Day, the surgeon appointed by the jury to inspect the body, should remain in the room while the other witnesses were examined.

Mr Wakley then addressed the jury, and said—Gentlemen, in this case I have to entreat you to dismiss from your minds everything that has been said and published respecting the death of the deceased—a case in which the calmest and most studied attention should be paid to the value of an oath. I entreat you to dismiss from your minds whatever you have heard out of doors, and that you will be governed not by your feelings about the practice of flogging, but that you will be guided only by the evidence given before you; and by being only so guided I am quite sure, from the manner in which you acted at the opening of the inquest, that you will act most fairly in giving your decision on the evidence, and that alone. I mean to commence the inquiry by taking evidence as to the state of the deceased's health on the day he was punished, and of his health before, and then after the flogging, and then to go on following it up to the day of his death; and after we have done so, then to go into the law of the case. Mr Day will remain in the room to hear the history of the case throughout.

Mr G. Clark applied for Lieutenant Ireland, the adjutant of the regiment, to be permitted to be in the room to instruct him.

A juror objected, on the ground that his presence would intimidate the privates who might be examined.

The coroner said he would take care that if any intimidation was used he would soon stop it.

A juror said if he was an officer in the regiment he would not stay in the room.

The coroner asked if the cats with which the whipping was inflicted were present; and being replied to in the affirmative, directed they should be brought in.

The following witnesses were then examined:—

Henry Potter examined.—I am hospital sergeant in the 7th Royal Hussars. The deceased died on the evening of Saturday, the 11th inst., in my presence and that of Dr Warren. One of the privates, of the name of Cook, lay in the next bed to the deceased, and on the other side was one named Frost. Deceased had at the time of his death kept his bed again for four or five days. He was Dr Warren's patient. Deceased was punished on the 15th of June last, and I received him into the hospital directly after he had received his punishment. Deceased walked into the hospital of himself. Fomentations were first applied to his back and afterwards dressings. He kept his bed for the first fourteen days; the skin healed readily. About ten days after he had left his bed his skin had all healed, and he would have left the hospital the next day had he not complained of a pain in his left side. That pain afterwards left his left side, and flew to his bowels. He again kept his bed for four or five days. On the Saturday of his death deceased was seen by Dr Hall, one of the medical staff in London, by request of Dr Warren. About 3 o'clock the same afternoon deceased became insensible, and remained so until he died, about 8 o'clock. Could not say who were the officers forming the court-martial. Deceased was four or five days under arrest before the court-martial was held. Deceased was a man given to drink. He had been before punished by extra drills, &c. He was not sober when he committed the offence. He was a native of Nottingham. None of his relatives visited him while in hospital. He was a man of very sound mind. He was in the hospital once before for a kick from a horse, but not for any punishment. He joined the regiment before I did. Never knew him to be punished corporally before.

The coroner.—You may retire for the present, but you will be examined again.

George Sparkes examined.—I am a private soldier in the 7th Hussars. Knew the deceased. Was present when he was punished. It was somewhere about the 15th of June last. The punishment was inflicted in the presence of the regiment drawn up in square in the riding-school, between 9 and 10 o'clock. Farrier-major Critton commenced the punishment with a whip. The man was fastened to a ladder nailed to the wall. He was bound by cords, and was stripped. His shirt was off. The word to commence was given by Colonel Whyte to Lieutenant Ireland, the adjutant, who gave it to the farriers. Dr Warren was present, and stood within three or four yards of the deceased. The adjutant asked the colonel if he should begin, and he said, "Go on." The first farrier gave 25 strokes, and the second took the whip. A Sergeant Patman counts the strokes and calls them out. When he had counted 25 the next farrier took the whip. Does not know how long 25 strokes took, nor how long the whole took. Each stroke took as long as a man would give two flourishes in the air with the whip. Farrier Evans was the second farrier; he also gave 25 strokes. Farrier Critton then took it again, and so on until 100 lashes had been given, when a fresh whip was taken, and the same farriers continued throughout until the other 50 lashes were given.

Five whips were then brought in and laid on the table. The handles were about 18 inches long, and the thongs nearly as long.

The coroner examined them, and said two had dark handles, and the other three white ones, and the thongs as well as the handles were heavier, and the cords and knots thicker and larger than the others.

By the coroner.—The whips are brought into the riding-school by the farriers covered over with a cloth. Cannot identify which of the whips was used on the occasion. Never had any of them in his hand before. Could not tell by the colour of the handles. (Examining them.) These two seem to be thicker than the others.

Examination resumed.—Deceased asked for water when the farriers were changed. The surgeon asked the deceased no questions. The deceased made no appeal to the surgeon while receiving the punishment. The change of farriers did not occupy a minute. Blood appeared between the shoulders before the first 25 were completed. There was a great discharge of blood before the whole was completed. It ran down into his trousers. Cannot say how much blood there was altogether. Deceased, when the 100 lashes were completed, said, "Lower, lower." That was the only word he expressed. Dare say the farriers struck lower hearing that remark, but no order was given by the commanding-officer or the surgeon. When the punishment was completed some water was thrown on his shirt, which was thrown over his back. He was then undone from the ladder, and marched by a corporal and a man from the riding-school to the hospital. Between 200 and 300 of the men were present at the punishment. Two or three of the men fell down while the punishment was given. One was a corporal and the other a private. All the officers were present; none of them fainted like the men. The surgeon did not examine the deceased's back after the flogging, or while it was going on. Had seen flogging before by the same farriers. Saw nothing different in the infliction of the punishment from other occasions. No persons but the military were present.

By the coroner.—After the deceased had left the riding-school, Colonel Whyte said something about its being a brutish affair.

The coroner.—Use the Colonel's own words.

Witness.—Colonel Whyte addressed the whole regiment in a loud voice, and said he was sorry that such a brutish exhibition should be performed in the regiment; but when the deceased came out of the hospital his comrades might tell him that whenever he was again guilty of such like offence he should have the same punishment, as he was determined to put a stop to such conduct in the regiment. The Colonel meant the conduct of White towards the sergeant. Heard the sentence read before the punishment was inflicted, but was not present at the court-martial.

The coroner.—The brutal exhibition you have mentioned, did you understand that to mean the deceased's conduct or his punishment?

Witness.—The Colonel said such brutal conduct towards a sergeant should not be permitted, and he was determined to put a stop to it.

Richard Cooke examined.—I am a private in the 7th Hussars. Was present when deceased was punished. It was on the morning of the 15th of June. Generally speaking, deceased was considered of a delicate constitution, particularly in the chest. Never knew him to be under medical treatment before that. He had been in the hospital, but not under my notice. I only speak as to his general health out of hospital. Saw deceased fastened to the ladder by the hands and legs and arms. Heard deceased ask to be struck on the back, and not on the arms. That was after the first 25 lashes. After the first 25 lashes the part gets deadened, and every blow on a fresh place gives fresh pain. Had seen flogging for nine years. Both the farriers had frequently flogged before, both in America and in this country. Heard no remark from the deceased while being punished. The surgeon did not interfere while the punishment was going on. Cannot identify the cords, as I was not near enough. One of the cats had the cords stained. All these have been stained. The flogging was at quick time. Think that it took about half an hour. The farriers flourish the cat in the air between the blows. The farriers strike as hard as they can strike, and if they did not do so they would be liable to be punished themselves. The punishment in our regiment is more severe than in any other in the army. In our regiment it is done by the farriers, while in other regiments it is done by the trumpeters, who are boys. Farriers, from their being men, strike harder. Had seen an extraordinary degree of punishment in this regiment while on service in America, but not so much in this country. The surgeon did not interfere. A stop takes place between every 25 lashes, in case the colonel or surgeon should interfere. The surgeon did not, and the colonel stood with his arms folded. The blood was flowing down deceased's overalls when he was removed. About half-a-dozen men fainted. Generally some men fainted. Before the punishment was begun, the proceedings of the court-martial were read over in the presence of the deceased. He was not handcuffed.

Mr G. Clark, after the witness left the room, requested that he might be recalled, in order that he might be asked what Colonel Whyte had said to the regiment after the punishment was over.

Cooke recalled.—Colonel Whyte said it was some time since he had to resort to such unpleasant punishment, but that no excuse for an assault of that nature could be offered. He said so, alluding to the offence with which the deceased was charged. The deceased being drunk at the time he said was no excuse for such an assault, for drink only showed the brutish nature or the brutality of the man who committed the crime. Colonel Whyte then addressed the men, and said, " And you can tell him, when he comes out of the hospital, that at each time he commits a similar offence he shall have a similar punishment."

Thomas Pegg examined.—I am a private in the 7th Hussars; was in the hospital as a patient when the deceased was brought in for treatment for the injuries to his back. Had never before seen a man in that state. His back, from the nape of the neck to the bottom of the back, was wounded. He walked in. His bed was let down, and I and Riley, the orderly of the hospital, laid him on his face and sponged the blood from off his back. The blood was still flowing slightly. It was fomented during the day with rags in hot water. He complained of the severity of the punishment. Between the bladebones and the shoulders was the worst part. The skin was broken, and it was bleeding. I can't compare it to anything else but like a person who had been cut to pieces. It was a most severe flogging in my opinion.

A juror.—That's right my man; speak your mind.

Witness.—The surgeon was not there for an hour and a-half afterwards. Potter acted till Dr Warren came. Colonel Whyte and Dr Warren then came in. He did not examine the back or take the clothes off deceased's back. Neither of them spoke to the deceased. Cannot form an opinion as to the extent of the wound. As far as I can form a judgment of the wound, should say about four or five inches in breadth, and about six inches long. That was between the shoulders. Had only seen one flogged before. He had 150 lashes. The surgeon did not see the deceased again until the next morning about 10 o'clock. Colonel Whyte did not see him in the interval. On that occasion Dr Warren examined his back, and gave orders that he should continue the same treatment. Dr Warren did not feel deceased's pulse, or ask him any questions. Deceased did not speak to the doctor. Attended deceased for four or five days. The same treatment was persevered in. The back seemed better when I left. There was much discharged matter, and it smelt very much when the rags were taken off, and there was great pain. The bad smell commenced about the second day, when the rags were taken off. The surgeon was present when the rags were taken off, once a day, after the first one.

George Skinner examined.—I am a private in the 7th Hussars. I was in the hospital when the deceased was brought in. He lay right in front of me. He walked in. He seemed quite cheerful, and was whistling when he came up stairs from receiving his punishment. I remained in the hospital until deceased died, and saw him die. The surgeon came into the hospital on the same day, but never went to or spoke to the deceased on that day. A man named Pegg attended upon him, and fomented his back. He did so for some time, and then applied cold lotion for skinning the back, and after that some salve to rub it. Corporal Ruth and private Hefrome——

The witness here said, "There is a person listening whom I don't want to hear what I say. It's Sergeant Potter looking in at the door."

The coroner directed the passage to be cleared, and the room door to be shut.

Witness.—Deceased undressed himself, and laid himself on his face. He felt very faint, and asked me to go down to Sergeant Potter, and ask him to oblige him with a cup of tea. I and Wood went down, and Wood went to see Potter. When he came back, Wood told deceased, in my presence, that the sergeant would not oblige him. Deceased made answer, "Could you not get it in somehow or other?" and Wood got a man named Cousins to go to the canteen to get some tea and sugar, and Wood made the tea for him in the kitchen. Deceased said he had not had any breakfast; and that flogging was a good breakfast for him. He had, on that day that he was punished, nothing to eat but bread and butter. The next day he had breakfast, which is generally about 8 o'clock. The surgeon came in at the usual hour on the second day, and looked at deceased's back, and told him to go on with the water. The surgeon saw him every day afterwards. Do not know what he ordered. The surgeon always wrote his directions in a long book which he brought up with him, and took down again to the surgery. Deceased was removed into the opposite ward on the Friday, and died next day, Saturday. He had lost the use of his limbs at that time, and it was against his consent that he was removed. He was removed because the surgeon said his was a medical case, and he was in the surgery ward. About the fifth or sixth day the deceased got up and walked in the fields opposite the barracks. He appeared very cheerful, and generally as men seem after being flogged. Deceased only complained of his legs on the Thursday or Friday before he died. At the end of about a fortnight his back was nearly healed. Saw his back; it appeared skinned over, but seemed inflamed about the half-breadth of my hand on each shoulder-blade. There was a separate patch on each shoulder. Deceased went on for about three weeks, and then, when the doctor came and asked him how he felt, deceased said he felt a singular pain in his left side—a pain which shot through to his back. The doctor said that might go away in a day or two, and appeared to pay no attention to it. Deceased complained of his heart beating violently, and said it was singular that his heart did not beat when the doctor came. Deceased was well enough to go out, when boils broke out on his back. Deceased was bled in his arm when he complained of his heart beating. Deceased first complained of his legs on the Friday before his death. Deceased had a blister put on his back on the Thursday night after he came out of the bath. One had been put on his chest before he went into the bath. The doctor came to see deceased on the Thursday night before his death, and told him to cheer up and not be so low. Deceased said something about the pain arising from the flogging, to which Dr Warren said, "Yes, I am aware of that; you will be as good a soldier as ever you have been; you have only struck a man you ought not." Saw two doctors come from town on the Saturday that he died; Dr Warren wanted deceased, who was lying on his back, to be turned over on his face, but the other doctor opposed it, and it was not done. Did not hear the deceased use the word "flogging" to the doctor. He murmured something which witness could not distinctly hear.

The book in which the doctor's entries were made in the hospital was here brought in, and identified by the witness.

Sergeant Potter said all the entries were not in that book, but the medical register would show it. That was also brought.

The coroner ordered him to refer to the first entry; and, having examined the first book, said he could not find any entry relating to White's case until June 28, and that, poultices to the back thirteen days after the floggings.

Mr Wakley, handing the book to Sergeant Potter, asked him to find the first entry.

Sergeant Potter said that arose from there being nothing but fomentations before that, during which time the doctor gave the directions to the deceased himself.

Owen Riley examined.—I am a private in the 7th Hussars, and am orderly in the hospital, and assist Sergeant Potter. Deceased was not under my care. Received orders from Sergeant Potter when deceased came in to take a sponge and wash his back and take the blood off. The back was bleeding from the effects of the cats. Have seen the backs of other men who have been flogged, and do not consider he had been flogged more than others, who had had the same number of lashes. Fomentations were first applied, and then poultices. The back was not quite healed when the poultices were left off. On the second day after the flogging deceased complained of a burning pain in his chest. He first complained of a change on the Tuesday before his death. On the Wednesday night I believe the doctor was sent for to him between 11 and 12 o'clock at night, who ordered a hot bath. I think he was sensible on the Thursday before he died. There is a cook in the hospital, but the sick cannot have what they like. The doctor orders what they are to have. Do not know what diet he had. The sergeant (Potter) can tell, if it is in the book. Deceased was not on the hospital book on the day he was admitted, and had the common diet, but afterwards he was under the doctor, who ordered the diet.

Charles Cook examined.—Am a private in the 7th Hussars. I was taken into the hospital on the 25th of June. The deceased was then up. Did not see deceased's back until two or three days before his death, when he was brought into the ward where I was. He was sensible when first brought in. He complained of a pain in his side. Saw his back. It was a little red. He had been blistered, and there was dressing on it. Above the blister it was rather rough when I felt it. Before he was taken worse he said to me that he was getting all right again. That was on the 26th, the day after I was admitted. He looked then rather more delicate than he used to look. I was allowed to get up after he came into my room. He said that Dr Warren appeared to be afraid of him now, and told him that his flogging was the occasion of his illness. That was on the day after he came in. M'Leod was present when deceased said that.

By the coroner.—That was two or three days before he died. He told me that the doctor was afraid of him now. I said, "What did he mean?" and deceased then said, "He told me that my punishment was the instigation of my illness."

John M'Leod examined.—Am a private in the 7th Hussars. Was in the hospital when deceased was brought. Had nothing to do with the treatment. Was present with Charles Cook on the day before his death, when some conversation took place between deceased and Cook. Deceased began by making a remark that he was afraid that the doctor thought that the lash was the cause of his illness. He did not say that the doctor told him so, but that it was his own thought. He did not say that the doctor had spoken to him about it. He said he was afraid the old man, meaning the doctor, thought the lash was the cause of his illness. That was the last I heard him say.

The coroner.—Is the skin here?

The summoning officer.—Yes, Sir, Sergeant Potter has it here.

The coroner.—Bring it in. Call the witness Skinner.

A small bottle was then brought in by Sergeant Potter, containing a portion of the skin in spirits.

The coroner, taking the bottle, took the skin out, and placing it on a piece of paper, asked Mr Day if he had examined it? who replied that he had.

Mr Wakley then spread the skin out on the paper. It resembled a thick piece of buckskin, and was handed from the jury to the end of the reporters' table where the witness Skinner was standing.

The coroner.—Skinner, you stated there were two patches of inflammation on each shoulder. Examine the skin, and see if that is like the parts where there was most inflammation.

Witness (examining it carefully).—I can't swear to the skin. Those were the parts where the most inflammation was. There seems not so much blood now on it.

The coroner (to Mr Day).—Mr Day, did you examine the skin?

Mr Day.—Yes, I did. There is less inflammation on the piece of skin since it has been immersed in spirits. I tried it on the deceased's back, and spread it out. It fitted on the right side. There is not the whole skin that was removed.

Sergeant Potter recalled.—Followed deceased into the hospital two or three minutes after he went there. Deceased asked me for some water, as he was very faint. Just as the sergeant came into the surgery some person called out in the passage and said something, but I was so busy that I could not attend to it. Could not give deceased any cordial or anything without the order of the doctor. Dr Warren says that no entry is in the book of the fomentations and poultices.

Coroner.—Now turn to the entry of the 28th of June, and read it.

Witness.—Deceased was kept on low diet, viz., quarter of a pound of meat, half a pound of potatoes, and three-quarters of a pound of bread. I do not know how long he was on low diet, but it will appear in the register. (Referred to register). He had low diet from the 16th of June to the 19th, and from the 20th he had half diet to the 9th of July. Half diet consists of a pound of bread, two pints of tea, half a pound of potatoes, half a pound of fresh meat, and a pint of soup.

The coroner.—Was he ordered any medical diet?

Witness.—He was ordered medicine when he became ill. He told me he was ill on the 5th of July, but his diet was not changed then. He complained of pain in his left side. I reported it to Dr Warren on the following morning. This small book is my guide, and is kept in the surgery. On the 28th of June, fomentations were ordered at night, and poultices in the morning. That was to both the deceased and Mathewson, who had been punished on the 22d of June. The next entry in the small book is on the 1st of July. It is, "poultices to boils." There were four or five boils broke out on the deceased's back. One large poultice covered them all. That was for White alone. That is the whole of the entry. [The witness continued to read extracts describing the treatment deceased had experienced, from the time of his admission to the hospital to his death.]

John Thomas Mathewson examined.—I am a private in the 7th Hussars. Went into the hospital on the 22d of June, and slept in the next bed to the deceased. That was a week after he was punished, and continued there until after he was removed into another ward. He was removed on the Friday, and he died on the Saturday. He complained on the Saturday week after I went in of a pain, a singular pain in his chest and back. That was a week before his death. Was present when he described the pain to the surgeon. Dr Warren said to deceased, "How are you, White?" That was on the Wednesday before his death. Deceased made no answer. Dr Warren immediately said, "Come, cheer up, it was for no disgraceful crime that you were punished." White then said, "This is through the lash that has caused my illness." The doctor made answer, "I know it is," three several times. He did not explain what he meant by this. I understood that he meant it was through the severity of the punishment. The doctor then went away. Deceased had his shirt off at the time, and he examined his chest. He said it was through the other. I thought he meant the flogging. Dr Warren, when he examined deceased, did not put his ear or tap deceased's chest. Saw Dr Warren come again the same evening, and bled him, as he did in the morning, and he blistered him. He did not go near to examine him, but only looked at him as he pulled off his shirt. He ordered him a bath, which deceased got about 1 o'clock on the Thursday morning. Deceased was quite sensible then, but had been partly unconscious before. On Thursday morning Dr Warren came about 10 o'clock, and told deceased to cheer up, and he would be a good soldier yet. He then looked at deceased's chest and back, but did not go near him, or use any instrument. He ordered him a blister if he felt worse, and saw him again in the evening.

By the coroner.—Saw nothing further as to the cause of deceased's illness. Deceased eat his allowance of food till the Sunday before his death. On the Monday I made him some toast, but he could not eat it. He only eat one potato that day. Told Dr Warren on the Monday that deceased had said his appetite failed him. Dr Warren said that would get better, it was only from the medicine he had taken. He ordered nothing for deceased; no nourishing food. Heard deceased say several times that his illness was through the lash, that he was not in a fit state to be punished, and that the doctor knew that fact.

The coroner here suggested to the reporters that the witness's name, as he had been punished, had better not be given, but

The witness said, "I would rather that it should be mentioned, and that publicity should be given to the fact."

The coroner here addressed the jury, and said, "Now, gentlemen, there are two other parts of this case. It necessarily divides itself into three parts. I will put a few questions to the witness, and will then relieve you for a few minutes, or for this day altogether."

By the coroner.—Saw the punishment. Was a prisoner at the time and taken in to see it. Had seen the punishment before, but not in the 7th Hussars. Deceased had not his stock on. Dr Warren and Colonel Whyte stood by, one on the left and the other on the right. Neither of them went to the deceased at all to examine him while the punishment was going on. Deceased never moved from his position. All he said was, "Strike lower," as the lashes took effect on his neck. Saw the marks on his neck after he was struck there. The skin there was not cut but raised up. The cats that deceased was punished with had been used before. They had dark handles.

The witness here took up the cats, and compared the light ones with the dark ones, and said that the dark ones were the heavier. He then showed the one he had been punished with.

By the coroner.—I was punished for insolence to a sergeant, in calling out "Halloa," in a stable. He did not come in and call me. When he came in, he said, "What do you mean by answering that way?" It was Sergeant Temple. I said "I did not know who called me;" but he continued reprimanding me, and I said, "Do you want me to go down on my knees to you?" He then ordered me on fatigue duty. I was then taken up before Colonel Whyte, and he ordered me into a week's solitary confinement. I asked him what way I was to answer? and he ordered a court-martial for insolence to my commanding officer, and I had 100 lashes. When I went into the hospital the doctor never came to see me until the next morning. I went into the hospital at half-past 12 o'clock. White, the deceased, dressed my back. Saw the surgeon the next day at half-past 10 o'clock. He ordered me nothing more than lukewarm water to my back for six days. Came out of the hospital this day. My back is not well now, properly speaking. Had boils on my back and pains in my side, back, and chest, the same as White complained of. Had great pain in my chest the next morning, and difficulty of breathing, attended with pain. My back was dressed, but White's was not. Three or four days afterwards the pain left the chest and came to the sides. It would sometimes leave for two or three hours, and then come back again. Had no medicine for it. Feel it still in the evenings, when I lay down and draw breath; it feels as if something was running into my sides. Did not mention it to the surgeon.

The coroner.—Did you observe any difference in the treatment of White, which you did not observe in the treatment of other men?

Witness.—No, I did not. Believe there is an order for the removal of the stock from the neck while under punishment. Was going to keep mine on, but took it off by order of the adjutant. I had only been seven weeks in the 7th Hussars when I was punished. Had formerly been in the artillery. Some of the lashes fell on my neck and amongst my hair. The marks are there now. The same farriers flogged me.

The coroner then said, I have a very strong feeling in this case that it would be proper to examine the spine. I don't know, Mr Day, that it will be necessary, but we'll be guided by the opinion of the jury on the subject; and I think it is most proper that a most searching inquiry should be made into a case of this nature, and that it is most important that the spine should be examined, after the evidence we have had about deceased's legs.

Mr Day said—I do not think it at all necessary. I saw enough in the body to account for death. I think, therefore, there is no necessity for examining the spine.

The coroner.—I think it is most important that the spine should be examined.

At a quarter past 3 o'clock the room was cleared of strangers, and on the public being re-admitted at a quarter to 4, it was announced that the inquiry was adjourned until Monday next, and that, in the meantime, the body will be exhumed for the purpose of further examination of it being made by Mr Day, assisted by an eminent anatomist from town, whose name did not transpire.

The inquest, adjourned from the 20th, upon the body of Patrick Frederick White, a private in the 7th Hussars, whose death, it was alleged, was caused by excessive flogging, was resumed yesterday, at the George the Fourth Inn, Hounslow Heath.

Mr Wakley, the coroner, arrived from town at 9, and took his seat at half-past 9 o'clock, when the inquiry was immediately proceeded with. The room was densely crowded.

There were present, Mr Clarke, solicitor, of New Brentford, who, as on the former occasion, appeared on behalf of the regiment, Colonel Whyte, Lieutenant Ireland, the adjutant, and several other officers of the 7th Hussars.

It was at once arranged to continue the evidence.

James Elsworth, a private in the 7th Hussars, and who was present at White's punishment on the 15th of June, was first called. The statement which he made was, that the punishment first inflicted on deceased by Farrier-Major Critton, had been exceedingly severe, the farrier, as he described it, rising on his toes, striking with all his strength, at each lash, and that, when Evans, the second farrier, struck with less force, the adjutant, who stood on the right of the colonel, said threateningly, "Do your duty." The words of the colonel when, the punishment being over, he addressed the regiment in the square, were, according to Elsworth's version,—"That it was a long time since he had had to resort to such unpleasant punishment, and he was sorry to have to do it now; but the man's having been drunk was no excuse for his insubordination. It was not the first time that he had made these brutish attacks; he had merely done it as an example to the young soldiers, and his comrades (alluding to the deceased) could tell him when he came up that he would, every time he committed the offence, receive the same amount of punishment as on this occasion." This witness had been present when the offence for which deceased was punished was committed. It was at Hampton Court barracks at 9 o'clock at night. There were in the room the deceased, Sergeant Daley, Thomas Hollis, corporal, William Harvey, private, and several other men who were in bed. Witness saw deceased go from the side of the bed up to the sergeant and strike him with a poker on the breast. The sergeant retreated, and deceased striking at him a second time, missed his aim and fell down. Hollis then, by the sergeant's order, seized White, who, appearing to be a little intoxicated, was without difficulty secured. He was taken to the guard-room. Elsworth was not called before the court-martial as a witness. The deceased was kept under arrest until he was tried. The sergeant was heard to say that he had been hurt by the blow inflicted by deceased. The court-martial was held at Hounslow-barracks. Sergeant Daley, at the time he was struck, was orderly-sergeant and was calling the roll. Deceased was, in general, a sober man; he had got drunk on this occasion at the canteen. Elsworth had been present at three floggings in his regiment, at one of which the colonel was present. The first case was one in which 100 lashes were sentenced, and 75 only inflicted; and in the second and third cases 150 were sentenced, and 50 only inflicted, the Commander-in-chief remitting the remainder on the ground that corporal punishment had previously been suffered by the culprits. Dr Warren was present at the two latter cases.

The coroner here put to Elsworth the question—"Did you notice, in the manner of tying White, or in the treatment generally, or in the manner of striking, anything different from what you noticed in any of the three cases you mentioned?"—Yes, I don't say that the punishment was so severely inflicted on others as it was on White. The farriers struck harder. In the first case referred to, the man was able, the day after the punishment, to go on duty. There was more blood drawn at White's punishment than I ever saw at any other flogging. The same farriers flogged in all these instances. During the whole of the time of White's flogging, neither the colonel nor the doctor went near him. The doctor never moved, in the half hour during which the flogging lasted, from the position which he first took up.

The coroner, at the request of Mr Clarke, asked the witness how many times he had been convicted of desertion? The answer was—"Twice; once because I had been ordered to be flogged. I have been in the regiment five years, and I have been flogged seven times. Many men in the regiment have been tried for desertion; many have deserted more frequently than I have. Since 1844 I have not been punished. Lately I have been recommended by Colonel White, on account of good conduct, for promotion. No complaint has been made against any man in the regiment for the evidence he had given at the inquest. Mathewson was at present under arrest; but not, he (Elsworth) believed, on that ground. He had been in confinement when he was called to attend the inquest."

The two cats used at the flogging, and three others, were here produced. The knotted lashes of those recently used were still discoloured with blood; but, of the five, those with which the deceased was punished seemed to be the lightest.

John Mathewson, who had before given evidence, was called in, and was asked if he had been subjected to any punishment in consequence of the manner in which he had given that evidence.—He replied that he had been ordered to be confined to barracks until the inquest was concluded. The punishment for which he had been confined, when he came to give evidence, would expire this week; but an order had been issued that he was, after that, to remain under arrest.

The coroner then said, If you are subjected to any punishment in consequence of what has taken place here, be sure and let me know it.

The coroner here intimated to Mr Clarke that he considered it absolutely necessary that they should have the evidence of one of those men by whom the punishment had been inflicted on deceased, but that he would not call either Colonel Whyte, the adjutant, or Dr Warren, as witnesses. Should those gentlemen, however, desire to make any statement to the jury, it would be gladly received, and, of course, taken down as evidence.

Mr Clarke wished that Dr Warren should be called as a witness.

The coroner said, that as Dr Warren now stood, in consequence of the evidence which had been heard, he could not be sworn as a witness. They had only to ascertain if the wounds inflicted were the cause of death to the individual, and they must go into the circumstances in order to know whether or not the punishment was a legal punishment.

The Rev. H. Scott Trimmer, the vicar of Heston, was briefly examined. —He had, on Monday the 13th instant, received an application from a sergeant-major in the 7th Hussars, respecting the burial of the deceased. The statement was that liver complaint was the cause of death; but on being questioned, the sergeant admitted that the man had been flogged five weeks before, and that there had been some reports of his having died in consequence of the severity of the punishment. The rev. gentleman asked for the coroner's warrant, and, finally, refused to perform the funeral service until some communication reached him from Colonel Whyte. Not having received any, he sent the following letter to the coroner:—

"Sir,—Application having been made to me yesterday respecting the funeral of a soldier who now lies dead at the barracks at Hounslow-heath, I made inquiry of the sergeant who brought the message as to the cause of the man's death, which he stated to have been disease of the liver, and said that it had been so ascertained by surgical examination. I told him, a report having reached me that the man had lately been flogged, I wished to know whether it was true. He said that five weeks since the man had received 100 lashes, but that he had recovered so as to have been pronounced fit for duty, and that he would have gone to his duty at this time had he not been attacked with the liver complaint, of which he died.

'I directed the sergeant to make known the commanding officer my opinion—that, under all the circumstances of the case, and looking at the report which was abroad, it was important that an inquest should be held previously to my being called on to perform the funeral service.

"Four o'clock to-morrow afternoon is the time named by the sergeant as the hour appointed for the funeral. I think it my duty, therefore, to apprize you of the case, in order that you may judge whether or not an inquest will be necessary, and that no time may be lost, as it will, of course, be important, owing to the heat of the weather, that no unnecessary delay with respect to the interment should take place.

"I have the honour to be, Sir, your most obedient servant,

"H. S. Trimmer.

"*Heston Vicarage, near Hounslow, July* 14, 1846."

A certificate signed Dr Warren was then brought to him from the barracks, that the man had died of a complaint of the heart. Mr Trimmer expressed his surprise at this different account of the matter from that which he had before received, and said to the orderly, by whom the message was conveyed, that the matter was in the hands of the coroner.

Mr Lewis, registrar of births, deaths, and marriages for the district of Isleworth, was called to prove the registering of White's death. The application was made on the 14th instant, and the certificate shown to him was the same as that produced to the Rev. Mr Trimmer. It was in this form—"Patrick Frederick White, 7th Queen's Own Hussars, died 11th of July 1846. Age, 26 years and 11 months. Disease, inflammation of the heart.—James L. Warren, M.D., surgeon 7th Hussars."

W. Brent, a constable, stated that he had been sent by Mr Pownall, a magistrate, to see Dr Warren relative to the death of deceased. Dr Warren told him that White had been flogged, but that he had died, not in consequence of that, but of disease of the heart.

Richard Evans, one of the farriers in the 7th Hussars, was then called. —He had received his orders to assist at the flogging of deceased from Farrier-Major Critton. The order, after each 25 lashes, was "Go on" from the adjutant. He never heard any such order while flogging as "Do your duty." No complaint was made that he had struck too severely. It was always left to the farrier how hard he should strike. He had flogged eight or ten men. There were never any orders given.

The coroner said he did not understand this. Who gave the orders?

Colonel Whyte said he knew of no orders.

Mr Clarke.—He does his duty as well as he can.

The coroner.—This a very awkward business, if the man has no orders. If I thought he would have given any such answers I would never have examined him. His own evidence is that he struck this man without orders. We are inquiring into the cause of the death, and by this man's statement it appears that he has inflicted wounds without order.

Mr Clarke.—The orders were general.

The coroner.—But I can't hear any more of this man's evidence for the present. All I can say is, that if there are no orders you have done very wrong to put him in this position. He may retire.

Dr James Warren, having first been informed by the coroner that he was not bound to answer any question, but that whatever voluntary explanation he desired to make would be accepted and taken as evidence, stated that he is a surgeon in the 7th Hussars, and had been present when the punishment which was alleged to have caused death was inflicted on White. He examined White most minutely on the morning of his trial, but had not made any entry in the medical books of the circumstance. The examination was expressly to ascertain if the accused was in such a state of health as to undergo corporal punishment. He forwarded to the Court a certificate, that the man was "in a good state of health, fit to undergo imprisonment or corporal punishment." He had not made out the certificate with any reference to the extent or amount of punishment: that was not his business. He next saw White on the morning of the punishment, but he made out no new certificate. He had not at the punishment interfered; he had given no orders, it was not necessary that he should do so. The deceased suffered no convulsions while being flogged. He did not faint, and he never appealed to him. He stood close to deceased. He asked him no questions, and never felt his pulse. Saw deceased next morning at 10 o'clock. Made an entry every second or third day respecting the state of his back, and his progress to recovery. For the first two or three days after he came into hospital there was no change made in his diet. On the 20th of June the diet was changed to low diet or half diet, and he continued upon half diet to the 6th instant; the alteration then was to spoon diet, three pints of tea. The scale of diet is regulated by the medical board. From the 16th to the 22d fomentations were used. On the 22d a cutaneous application, with acetate of lead, was resorted to. On the 28th boils having appeared poultices were ordered, and opening medicine was for the first time taken. The back healed rapidly for the first two weeks. At the end of two weeks it was nearly quite well. On the 4th of July the entry in the hospital memorandum book was "His back is now quite well, and he is fit for duty." On the 6th deceased made complaints of pains at the heart. There had been no previous complaint of internal pains. He was not examined by the stethoscope.

The coroner here read Mr Mathewson's evidence relative to the conversation which was stated to have taken place between deceased and Dr Warren, in which Dr Warren was alleged to have said, in answer to White's remark that "his illness was through the lash," "I know it, I know it."

Dr Warren.—"A falsehood, Sir, a falsehood."

Further portions of Mathewson's evidence were read to Dr Warren, and that gentleman said he did not believe that the man had told the truth.

The statement was then resumed.

Dr Hall visited deceased; and the day before he died the deceased was removed to the medical ward. The *post mortem* examination was made by Dr Reid on Monday the 13th inst. The entry in the book did not particularize the exact name of the disease of which the deceased died. He was treated for inflammation of the pleura of the heart. Every soldier who dies is examined. Dr Reid is an assistant staff-surgeon. There were found, on examination, old and strong adhesions on the right side of the heart, and inflammation and recent adhesions on the left side. The *pleura pulmonalis* and *pleura costalis* were inflamed; the lungs were not inflamed. The pulmonary artery and the aorta were inflamed for an inch and a half; the pulmonary vein was very slightly inflamed. The liver was much enlarged, but healthy; the kidneys were quite healthy. There was no disease of the bladder, and the stomach and intestines were quite healthy. There were no diseased appearances in the chest. The inflammation extended from the heart to the larger vessels. There was no inflammation of the pericardium. The cause of death was inflammation of the heart and pleura, but it was impossible to say what was the cause of that inflammation. Was not present when the body was re-examined by Mr Day.

The coroner.—Do you know, Dr Warren, why it is that when the men are flogged their stocks are taken off?

Dr Warren.—Indeed I don't know; but the stocks always are taken off.

At this point in the proceedings an adjournment for an hour took place. At the end of that time the coroner again took his seat, when

Dr J. Hale was called, and, being sworn, said—I am a graduate of the University of St Andrews, and Fellow of the Royal College of Surgeons in London. I am a first-class staff-surgeon in the army, and reside at 45 Duke Street, St James's. I saw the deceased before his death on an order from Sir J. M'Grigor, and arrived here about 20 minutes after 7 o'clock. I met Dr Warren in the barrack-square. He had written to the Inspector-General, and we went at once to see the deceased, who was then in a dying state. He expired in my presence at a quarter past 8 o'clock. Dr Warren told me he had received 150 lashes, and that he was suffering from inflammation of the chest. Treatment was then quite out of the question, the pulse having ceased to beat at the wrist. I took an opportunity of examining the back of the deceased, and I also read Dr Warren's notes of the case, so as to make a report of the case to Sir J. M'Grigor on my return the same night. On the following Monday I got an order from Sir J. M'Grigor, requesting me to be present at the *post mortem* examination, and, considering the case to be one of interest, I brought Dr Reid with me. Up to that time I had not heard the probability of an inquest discussed, nor had it occurred to me. Dr Reid made the dissection for Dr Warren, and I saw the operation, and took notes of it.

The witness here entered into a detailed account of the appearances which the body presented on the *post mortem* examination, but as they are very concisely set forth in the following documents afterwards read and put in evidence, we omit this portion of the evidence.

(Copy.)

" *London, July* 11.

" Sir,—I have the honour to report to you that on the receipt of your order of this afternoon (dated 5 o'clock,) I proceeded immediately to the cavalry barracks at Hounslow, and visited the hospital of the 7th Hussars with Dr Warren, the surgeon of that corps, for the purpose of seeing Private Frederick White, the subject of his letter to you of this date, whom I found in a dying state, and who expired in my presence at half-past 8 o'clock.

" White, it appears, received a corporal punishment of 150 lashes on the 15th of June for military insubordination, which was inflicted in the usual manner in the presence of Dr Warren, and without any degree of severity calculated to attract more than any degree of ordinary attention. His back healed kindly, and nothing occurred to mark his case until the morning of the 6th instant, the day on which he was to have been discharged from the hospital to his duty, when he began to complain of pain in the region of the heart, extending through to the back and shoulder blade, which increased in severity, notwithstanding the measures adopted by Dr Warren for its relief, until yesterday morning, when paralysis of the lower extremities and retention of urine were discovered, and he died, as I have stated above, at a quarter past 8 o'clock this morning. It would be difficult to give a correct opinion of the cause of this man's death without a *post mortem* examination of the body, and as the circumstances of the case are peculiar and interesting, I suppose a minute one will be made by Dr Warren, and the result reported for your information,—I have the honour to be, &c. John Hale, M. D., Staff Surgeon, First Class.

" To Sir James M'Grigor, Bart., Director-General,
Army Medical Department."

(Copy.)

London, July 13, 1846.

" Sir,—Having, in obedience to your instructions of this date, proceeded to the cavalry barracks at Hounslow for the purpose of being present at the *post mortem* examination of private Frederick White, of the 7th Hussars, who died on the evening of the 11th instant, I beg leave to enclose a statement of the morbid appearances that were discovered on a careful examination of his body.—I have the honour to be, Sir, your most obedient humble servant. John Hale, M. D., Staff Surgeon, First Class.

" To Sir James M'Grigor, Bart, Director-General,
Army Medical Department."

" *Post mortem* examination of private Frederick White, of the 7th Hussars, aged 27, made 42 hours after death, in presence of staff-surgeon Dr Hale, surgeon Dr Warren, 7th Hussars, and assistant-staff-surgeon Dr Reid.

" General appearance.—Body muscular, and not much wasted; marks of venesection at the bend of the tight arm; marks of a blister on the epigastrium, and of another on the back below the scapulæ; marks of corporal punishment across the shoulders, particularly over the right scapula, but the punishment does not appear to have been severe, and the part where it was inflicted was quite healed; back discoloured from the gravitation of blood since his decease. Thorax—right side—Old adhesions binding the lung to the ribs and diaphragm throughout its whole extent; left side—inflammation of the pleura with recent adhesions, and effusion of serum, containing shreds of lymph to the extent of 12 oz.; lung engorged and infiltrated with serum. Heart—Muscular tissue soft and friable throughout; endocarditis on both sides of the heart, the inflammation extending some distance along the pulmonary artery, and over the valves of the aorta; *chordæ tendineæ* of the tricuspid valve matted together with fibrine; pericardium healthy, and not containing more than its natural quantity of fluid. Abdomen—Liver enlarged, and extending three inches below the margin of the ribs, but not altered in colour or structure, though there are old traces of inflammation on the peritoneal covering of the right lobe; gall bladder small, and containing a portion of pale-coloured bile; kidneys, bladder, and spleen healthy; intestines distended with gas, but healthy. Head—*Dura mater* healthy; *pia mater* injected, and the *tunica arachnoides* opaque in several places, similar to what is generally found in confirmed drunkards; lateral ventricles capacious but empty; *plexus choroides* enlarged, and the veins running along the floors of the ventricles distended; substance of the brain marked with bloody points when cut into, and softened at one point on its under surface. A portion of integument was dissected from off the shoulders and spine, where he had been punished, and all the parts underneath were found perfectly sound and natural. The integument itself, with the exception of some discoloration of the *cutis vera*, was quite healthy.

" July 13. JOHN HALE, M. D., Staff Surgeon, First Class.

" Copy of a Certificate which was drawn up and signed by the party present at the *post mortem* examination, for the satisfaction of the officer commanding the 7th Hussars.

" *Cavalry Barracks, Hounslow, July* 13.

" Having made a careful *post mortem* examination of private Frederick White, of the 7th Hussars, we are of opinion that he died from inflammation of the pleura and of the lining membrane of the heart; and we are further of opinion that the cause of death was in nowise connected with the corporal punishment he received on the 15th June last.

" JOHN HALE, M. D., Staff Surgeon, First Class.
" J. L. WARREN, Surgeon, 7th Hussars.
" F. REID, M. D., Assistant Staff Surgeon."

The skin was here produced, and Dr Hale stated that about half had been removed.

The witness.—The missing portion is of the same character as that I have in my hand. The boils were on this portion of skin, which was not broken, at least the cuticle or the true skin was not. The skin was removed not to conceal anything, but to see the condition of the parts underneath.

On the order of the coroner, who put a question to the witness with the view of ascertaining how far the skin was broken, the following entry by Dr Warren was read and put in evidence:—

" Patrick Frederick White.—This man has been removed to hospital after having received the punishment of 150 lashes. Skin a good deal lacerated and swollen, and covered with blood."

Witness (in continuation).—I have seen the history of the case given in the register, and I should ascribe the inflammation to the change of temperature at the time the deceased was recovering. A change of weather bringing rain and cold winds after great heat, took place in the end of June. A corporal in the regiment I know died yesterday from inflammation of the lungs. I do not think that the death of White had any connection with the punishment he received.

Mr Wakley.—Dr Warren denied the existence of pneumonia.

The witness.—The room where the *post mortem* examination was made is a small one, and as Dr Warren's sight is not good he might have overlooked the inflammation in the lungs.—The witness here expressed some disappointment at not having been allowed to be present at the subsequent examination of the body.

The coroner.—You must not attribute that proceeding to any want of respect to you Dr Warren. The fact was this:—Some of the gentlemen of the jury intimated to me the advisability of having the witnesses who were to make the medical examination as unconnected as possible with the barracks of the regiment. The order was therefore issued for Mr Wilson alone to make the examination, and it was deemed the best course not to allow to be present any of those medical gentlemen who had, in the first instance, examined the body, who had already formed their opinions as to cause of death, who had made out a certificate for the registrar, and who must, therefore, have prejudged the case.

Witness.—I have seen a great number of men punished during my long experience in the army, but I never heard such complaints made before of the consequences produced by flogging. I was struck by what Mathewson said, and having since examined him I found that his chest and lungs were quite healthy.

The coroner here intimated to Dr Hale that he should not like finally to ask his opinion with respect to the cause of death, until he was in possession of all the facts of the case; that he wished him to have an opportunity of hearing the other medical testimony and the depositions of the witnesses, and that before the inquiry was closed he would again call on him to state his views on the subject.

Dr Francis Reid was the next witness called and examined.—He said, I reside at 24 Stockbridge Terrace, Pimlico, and I am staff-assistant-surgeon. I am an M. D. of Edinburgh, and licentiate of the College of Surgeons there. I did not see the deceased during his life, but made the *post mortem* examination, and have been in the habit of doing so for the last 13 years. I have heard Dr Hale's evidence and agree in it. I look on the endocarditis as the immediate cause of death. The lining membrane of the heart was of a deep red, and this was not from the imbibation of blood, for it was not present; nor was it from decomposition, for the heart was not decomposed. The membrane was thickened by infused fibrine, and soft and slightly adherent. The membrane itself was readily torn from the heart, showing the decreased vital cohesion, and there were polypiform concretions of uncoloured fibrine adherent to and interlacing the fleshy columns and the *chordæ tendineæ*. That proves the inflammation to have been recent. In the left lung there was pneumonia in the first stage, but I do not attach much importance to that. The marks of pleurisy on the right side, and of inflammation in the liver, were of old standing.

The coroner.—Do you consider, Dr Reid, that the flogging which the deceased underwent had any thing to do with the state of disease you have described?

Witness.—There is nothing more difficult than to say what is the cause of a certain disease, for it generally originates from causes which are indescribable. It is as difficult to say what originated disease in the case of the deceased, as it would be in that of Corporal Barnet, to whose death, yesterday, Dr Hale has alluded. It appears that the man had got up and was so far recovered as to be able to do ordinary duty, and I think it probable that he might have caught cold at that time. But though I cannot say what caused the illness which terminated in White's death, I can say what did not cause it. I think the punishment had nothing to do with the cause of death. This is the first case which has ever occurred among all the men who have been punished at home and abroad, which has had a fatal termination, if it be so.

The coroner.—I am afraid your reading has not been very extensive on the subject, Dr Reid.

Witness.—I have heard of Governor Walls' case, in which a man was flogged to death there and then. The piece of skin I find is about one-half thicker than it was, and from that I conjecture that it is about one-half less in superficies. The skin of the back was loaded with blood, and as soon as I put my knife into it it contracted. When I took the piece out a beautiful clear red muscle was presented.

The coroner informed Dr Reid, for the reasons he had stated to Dr Hale, that he would require him again to give his opinion on the cause of death at the close of the inquiry.

Horatio Grosvenor Day, the next witness sworn and examined, said, I am a member of the College of Surgeons of England, and licentiate of the Apothecaries' Company. I live in Church Street, Isleworth. During his life I did not see the deceased man, but on the Thursday after the death I examined the body in the presence of my partner, at the barracks. I knew nothing of the history of the case previously. I opened the cavity of the chest and abdomen, and found the parts very much decomposed, and, of course, out of their usual position. The heart appeared rather smaller than usual, and of a softened texture. It had been cut open in each direction. I saw nothing decided about the lining of the heart, though it appeared rather inflamed and redder than usual. The lungs appeared gorged, particularly the left side. There were traces of inflammation on the left pleura, but I cannot speak of adhesions, as they had been torn through. The portion covering the ribs was more inflamed than that covering the lungs. The body was in a very decomposed state, and it was difficult to make observations. The liver looked larger than usual, and I thought rather paler. I have heard all the evidence thus far, and I think the cause of death was pleurisy and pneumonia. I suppose the death was caused by change of temperature and exposure to cold.

The coroner gave to Mr Day a similar intimation to that which he had given Dr Hale and Dr Reid, that he would again be called on to give evidence when all the facts had come out.

Mr Erasmus Wilson, the next witness called, said—I reside at 55 Charlotte-street, Fitzroy-square, and my profession is that of a surgeon. I am a Fellow of the Royal College of Surgeons, Edinburgh, Consulting Surgeon to the Royal College of Surgeons of London, and Lecturer on Anatomy and Physiology. Some years ago I was Demonstrator in University College. I have written a work on diseases of the skin, and another work entitled *Healthy Skin*, which I am happy to say is out of print. Mr Wilson then read the following statement of the observations made by him on examining the body of the deceased, and the conclusions which he drew from those observations:—

"On Wednesday, July 22, I made a post mortem examination of the deceased, my attention being especially directed to his back and spine. On the skin over each shoulder there were marks of lashes, and on the right of the middle line between the shoulders there was a large gap occasioned by the removal of a portion of the skin. A small bottle containing a piece of skin was handed to me by the sergeant of police. I took the skin from the bottle, and found that, though much shrunk by immersion in spirits of wine, while the gap from which it had been removed was stretched to its utmost, yet that it corresponded with the gap, with the exception of the side nearest the middle line, where a part had been cut away and lost. I was informed that the last piece had been cut away in order to make the remainder sufficiently small to enter the bottle. From the position which the last piece occupied, I believe it was more protected from the lashes than the preserved portion, and, therefore, being less interesting in a medical point of view, had been cut off. On the preserved portion of the skin there had been several marks made by lashes; the marks were red, and, upon cutting into one of them, I found that the redness, which was indicative of inflammation, extended through the entire substance of the skin. On raising the muscles or flesh from off the ribs and spine, I find a part of the deepest line of muscles, viz. that which lay in contact with the bones, in a state of disorganization, and converted into a soft pulp; in medical language, I should call this a pulpy softening of the muscles. The seat of this pulpy softening was the sixth and seventh ribs, near their attachment to the spine, together with their intervening space and the hollow between the sixth and seventh pieces of the spine. The extent of the disorganization was about three inches in length, by about one inch and a half in greatest breadth, and between a quarter and half an inch in thickness. In the space between the ribs the muscles had undergone this pulpy alteration, even so deep as the lining membrane of the chest, the softened muscles being in absolute contact with the lining membrane; that portion of the flesh which occupied the groove of the spine, and had undergone a similar disorganization, was one of the little muscles known to medical men under the name of the *multifidæ spinæ*. In addition to softening this little muscle, it was partly surrounded with blood. It was in a state medically called ecchymosed; the interior of the spine was in a state of extreme decomposition; the tissue between the spinal canal and spinal sheath was filled with a dark-coloured fluid, resulting from decomposition; the sheath itself was smooth and polished in its internal surface—a state indicative of health; it was perfectly devoid of nervous substance, which had been converted into fluid by decomposition, and had flowed away. The nerves remained, and presented a healthy appearance, so that, so far as the spine is concerned, I discovered no indication of disease. Two questions naturally arise out of the preceding examination; first, what was the cause of the pulpy softening of the muscles? secondly, could the state of disorganization, preceding the pulpy softening, influence the disease existing in the chest? The cause of the pulpy softening I believe to have been the excessive contraction of the muscles taking place during the agony of punishment. This excessive contraction would produce laceration, subsequent inflammation of the muscle; and the inflammation instead of being reparative, would, in consequence of the depressed state of the powers of the nervous system of the sufferer, be of the disorganized kind which results in pulpy softening. Had the man lived, the disorganization of the muscles would, in time, have been repaired. As regards the second question, there can be no doubt, that, although the common cause of inflammation of the contents of the chest is cold acting in conjunction with physical or moral depression, and might have been the cause in the case of the deceased, yet the presence of a portion of muscle in a state of disorganization and inflammation, in close contact with the lining membrane of the chest, might be adequate to the production of the same effect. Certainly no surgeon would feel comfortable with regard to the state of his patient if he were aware of much dangerous proximity."

Dr Hale was here recalled, and asked by the coroner whether he had been informed of the discharge of matter, the offensive smell, and the boils on the back of the deceased.—He stated that he had not heard of any great discharge of matter, or of its offensive smell, but he did not attach much importance to these; and as to the boils they were easily explained by the abrasure of skin caused by the cat and the habits of the deceased.

The coroner explained that he had merely put the questions to ascertain how far Dr Hale had the facts of the case before him.

Mr Wilson was then re-examined, and said—What I have stated with regard to the disorganization of the muscles is a scientific observation connected with pathology, and one which has never been made before. It was such an appearance as I was far from expecting, and I am not surprised that military medical officers should not be aware of its existence, because I don't think it is stated in any work extant. The place of the change in the muscles, corresponding with the side of the chest on which there was inflammation of the lungs, I am since I came here still more convinced that the muscular disorganization in question took a prominent part in causing that inflammation. The pulpy softening was in contact with the bone, and from one inch to an inch and a half from the surface. I am speaking of parts on the opposite side to that where the skin had been removed. It was deeply seated, and caused by a rupture of the muscles from their own spontaneous contraction. There can be no doubt that the disease of the heart and lung, and its lining membrane, were the proximate cause of death.

The coroner.—What effect do you think the corporal punishment has had in causing the disease of which the deceased died?

Witness.—I think there can be no doubt the man would have been alive now, had it not been for the corporal punishment he was subjected to. I have just overheard a remark from Mr Clarke to a gentleman behind him (Colonel Whyte), that all the medical gentlemen have given but one opinion on the subject of deceased's death. Now I beg to state that none of the gentlemen knew the whole nature of the case till they had heard my evidence. There is another remark which has been made, viz. that if I had been pressed at the time of the examination to give my opinion with respect to the corporal punishment, I should have agreed with those gentlemen that it was not the cause of death. But I saw there what I was not aware of. The change I observed is a serious one, particularly if it results from punishment more than once inflicted.

The coroner.—Did the change resemble in its character that in the muscles of the heart?

Witness.—It was a more intense change than that, but it is a very difficult thing to say whether in character they were the same. I have lectured on anatomy for more than 12 years, and have opened between 500 and 1000 bodies. I have never seen in the muscles of the back any change of the kind.

Dr Reid here requested leave to put a question to Mr Wilson, and was desired by the coroner to do so through Mr Clarke.

Mr Clarke accordingly asked, How was it that the injury was confined to one side of the back when both sides must have been equally convulsed by deceased's struggles? Is it not probable that only one side having been injured, a bruise was the cause of the peculiarity noticed?

Witness.—There is an assertion there which is not proved. There was no struggling, and the appearance could not have resulted from a bruise, for the muscles were in a protracted position. A part of the change was in the groove of the back, which was protected. The man was too tightly fastened down to struggle, but there was involuntary and spasmodic action, and under that the muscle gave way. The whole of the muscles down the back were affected.

Mr Clarke.—It appears that he struggled when he struck the sergeant.

Mr Wilson.—I think the appearance was caused by the anguish of punishment, and not by what has been referred to.

The coroner, at the close of Mr Wilson's examination, informed him that as the inquest could not possibly be concluded that day, at their next meeting the depositions would be read over to him and the other medical gentlemen, in order to put him and them in possession of all the facts, and afterwards they would each be asked to give distinct opinions as to the actual cause of the deceased's death. He also said that something must be done with Evans. It would have to be proved whether he had acted with orders or not.

Mr Clarke would attend to that point, and would also be prepared to prove that the punishment was legally inflicted.

Mr Day re-examined.—I quite agree with Mr Wilson about the change of muscle, but I cannot go so far as to say that it accounts for death. I have never seen the appearance before, but it does not seem to me to have any connection with inflammation in the lining of the chest. I think the cause of the disorganization of the muscles is mere conjecture, and I do not connect it with the stripes from the lash, though it may have been from the struggling during the agony of the punishment. Confinement to the hospital, and the depression of spirits consequent on flogging, may have rendered the deceased more liable to disease.

Sergeant Wright proved the accuracy of the copies of the proceedings before the court-martial, which was a district one, entered in the court-martial book, which was examined by the coroner.

Lieutenant Ireland was then sworn, and on examination said—I am lieutenant and adjutant of the 7th Hussars, and was present at the infliction of punishment on the deceased. All cavalry regiments flog on the ladder, and for as long as I have been a soldier I never knew men flogged in any other regiment of horse except by farriers. When the Colonel gives the sentence, I order the man to strip, and he is tied to the ladder. We have never flogged a man with his stock on, and I never saw it done in the service. I gave no directions as to the farriers; but when the man was tied up the colonel gave me the order to go on, and I then said to the major-farrier, "Go on." When the first twenty-five lashes were over, and space was given in case the man wanted water to drink, I said to the next farrier "Go on." I said no more, and I never heard the men say anything. The doctor did not examine the man when under punishment, and it is not usual to do so unless the man faints.

The coroner.—Did you say anything more to farrier Evans than "Go on?"

Witness.—I gave no directions to farrier Evans except to "go on," which order is given to each farrier every twenty-five lashes.

The coroner.—It is a very extraordinary thing. Surely there must be some directions given to the farrier how he is to use his strength. I don't see anything in the army regulations directing you to give any orders to the farriers at the scene of punishment.

Witness.—It is expected that the farrier is to do his duty like any other man in the regiment. He gets no orders but the direction "Go on."

The coroner.—Supposing there were two men to flog who never did so before, what instructions would you give them?

Witness.—None but to carry out the sentence by flogging the man.

The coroner.—Ought they to strike hard?

Witness.—I only order them to "go on." I did not say in the case of White, "Do your duty" to any of the farriers, and I can bring plenty of evidence to prove that.

The coroner.—The jury want to know what is the farrier's duty.

Witness.—The colonel dare not tell a man to flog a soldier before he receives an order to that effect from the Horse Guards. I have seen many men worse flogged than White.

The coroner.—I am very sorry to hear it. Some appeal must be made in some court or other. Evans states that he had got no orders, but that he must strike at once.

Witness.—I receive the order from my colonel, and I tell the farriers to go on, and they know where to strike. They have generally flogged men before. I have seen the major-farrier do so, and I believe Evans has, though I have not seen him. The major-farrier generally puts the farriers where they are to stand. He probably is instructed by his predecessor. There was nothing different in the manner of flogging White from the practice usual in our regiment.

Colonel John James Whyte having requested, through Mr Clarke, to give some explanation in regard to the punishment, which, by the sentence of the court-martial, the deceased had suffered, stepped forward and said, I hold the rank of Lieutenant-Colonel of the 7th Hussars. I was present at the time the deceased was flogged. The words I used, as well as I can remember, were to this effect. After the man had been removed to the hospital I called for the attention of the regiment. I said that I deeply regretted the necessity under which I had laboured, of inflicting a punishment, so disgraceful to the man and so disgraceful to the regiment; a punishment, too, which was of such rare occurrence in it. I further stated that this man had brought it upon himself by a long course of the same particular crime for which he had now been punished; that within the last three years he had been brought before me, or before the commanding officer, four times for assaults; that he had now ended his catalogue of crime by a cowardly assault with a poker, upon an unarmed man; and that it was in consequence of such conduct that he found himself in the disgraceful position in which he had just been witnessed.

The coroner.—Did you give to the farriers any directions as to the mode of punishment? There were no directions with the exception of the words "Go on." When the sentence is read, if it is for flogging, the adjutant desires the man to strip, and he is fastened to a ladder by the farriers. The adjutant then touches his cap to me and says, "All ready;" I say, "Proceed;" the adjutant then turns to the first farrier and says, "Go on." At the end of every twenty-five lashes there is always a pause. If the man wishes for water or to have the cords, which may be hurting him, moved, he is accommodated; and that being over, the next farrier takes the whip and the adjutant again says, "Go on."

The coroner.—Supposing the farrier strikes very lightly?—If I saw that he would not execute the punishment, and that he was in that way insubordinate, I would certainly confine him. I never saw such a case; but if you ask me, if a man would not flog, and what I would do, then I tell you I would confine him.

The coroner.—And if he did not strike with all his force?—If I saw that a man would not execute the punishment I would confine him. A man cannot strike with all his force, for a cat, like a flail, cannot be used with force.

The coroner.—It is not then the strongest man that can strike most severely?—No; for instance, that man (Evans) who has been before you—he hardly struck at all. I never saw a man flogged more lightly than that unfortunate man into the cause of whose death you are inquiring. Evans is a very nervous, mild-dispositioned man, and always flogs lightly.

The coroner.—You took no notice of this?—No, of course not. That was my impression at the moment, that the punishment was being executed lightly.

The coroner.—Did Farrier Critton make any observation?—No, it was not his place to do so.

The coroner.—He did not endeavour to make up for the deficiencies of his companion? (Laughter.)—Not at all, so far as I know.

The coroner.—One of the witnesses says that Critton stood on tip-toe when he struck. Was that so?—I don't know. Some men naturally flog harder than others; but as I have said, there can only be a certain amount of force thrown into the blow.

The coroner.—But the officers don't often try it, do they? (Laughter.) —No; I never tried it myself; but I know I was devilish often flogged at school. (Great laughter.)

The coroner.—How is it that there is this difference, which is so perceptible in the lashes of the cats?—I never had a cat in my hand until to-day; but I have inquired, and find that some of them were made in Brighton, and some in Birmingham, and there may be a difference in Brighton and Birmingham whip-cord. The inclination of the farrier, you may be sure, always is to lessen, not to increase, the punishment. It can be a pleasure to no man to see, much less to inflict, a flogging.

A juror.—How many of your men fainted, colonel, during the punishment of White?—Four or five; but there were about 70 recruits present.

Colonel Whyte, in conclusion, contradicted the statement made by Elsworth, that he (Colonel Whyte) had recommended him for promotion. He had never done so, and it was ridiculous to say he had, for he possessed the power at once to promote a man in his own regiment, and had no other person to refer to.

Dr Hale and Dr Reid were here recalled, and stated, that their opinions were not changed by the evidence which they had heard from Mr Wilson.

Mr Wilson adhered to his view of the case, although, as Dr Reid had truly said, it was very difficult to point out the causes of the disease. He had no doubt, however, that the moral depression, caused by his punishment, took a very important share in the subsequent disease of the deceased, who, if he had received a double allowance of grog, instead of a flogging, would unquestionably be alive now. It would be wrong to say positively that flogging was the cause of death, because the intermediate cause might have arisen from flogging or cold; but it was quite certain that a man depressed by flogging would be more likely to take cold than if he was in a state of perfect comfort.

The coroner.—If you would agree that the flogging had nothing to do with the death of the deceased, there would be an end of the inquest, and we need not meet again; but if you can't do so, it will be necessary, by reading over the depositions, to try and get from you as distinct an answer as possible as to the cause of death. If the jury cannot get that, the whole will result into a case in which they must rely on their own common sense in arriving at a verdict.

Mr Clarke, on an intimation from the coroner, promised at the next meeting to be provided with the necessary proofs to establish the legality of the sentence of the court-martial, and it was, after some deliberation, unanimously agreed upon by the jury to sit again on Monday next, at half-past 9 o'clock.

The inquiry, adjourned from July 27th, into the cause or causes of the death of Frederick John White, private in the 7th Hussars, was resumed yesterday at Hounslow.

The jury having been sworn,

The coroner said that he had received a letter, signed by a tradesman in Brentford, of the name of Norminton, giving a statement of such a character that he had considered it necessary to pay some attention to it.

This letter was then read:—

"Sir, With a view to the furtherance of the ends of justice by searching investigation, I submit the following circumstances to your consideration, knowing that I cannot place them in better hands. On Monday morning last at 8 o'clock, the son of Mr Goom, newsman of Brentford, was in Fleet-street with his papers in his cart, coming towards Brentford, when a soldier of the 7th Hussars or Queen's own, asked him to give him a lift, as he wanted to get to Hounslow in time for the inquest. The boy told him to get into the cart, and hastened his speed, seeing the soldier was afraid he should be too late. On the road the soldier told young Goom, that about the commencement of the inquest he was told he might have a furlough for about five weeks if he liked, to go and see his friends, which he accepted. When he arrived in Kent among his friends, and told them what he knew about the flogging of White, his friends told him he ought to be at the inquest to state what he knew to the jury, and they made a subscription to furnish him with money to defray his expenses from Kent to London on Sunday, by a van. During this conversation they had got to Kensington-barracks. A soldier seeing this other soldier in the cart, called out, saying, "How are you getting on?" The answer was, "I am going to the inquest." But as they were driving into Hammersmith, a soldier, a sergeant from Kensington-barracks, on horseback, came after them at full speed, and called to young Goom to 'Stop. I command you to stop, or I will immediately give you both into custody.' He (the sergeant) then, after he had stopped the cart, inquired of persons passing by where the station-house was, and was informed up the lane. He then ordered young Goom to turn out of the road, and drive up to the station-house, which the boy did; but when they had got near it, the sergeant told the soldier if he would go on quietly to the barracks, he would not give him in charge to the police, but if he attempted to run away, he (the sergeant) would gallop over him. The boy was then permitted to come home with his cart and papers. The sergeant and soldier, he supposed, returned back to Kensington-barracks, and I have not heard anything further what became of the soldier, whether in prison or at large. I have just heard the soldier at the barracks called him by the name of Simmons.

"Yours, &c.,

"Old Brentford, August 1. T. Norminton."

The coroner.—This is not put in as evidence, but as a reason why I call the soldier who is said to have got into the cart. The writer has signed his name, and complaints might be made if I did not investigate the statement. It would be treated, of course, differently if it were an anonymous communication, and I have received at least 100 of these referring to the inquest; but as it is, I do not expect that anything will come of it.

James How, a private in the 7th Hussars, was then called, and stated that on Saturday week he left the regiment, without leave, to go home and see his mother. He had not been present at the flogging of White, and knew nothing at all about it. Had not told young Goom anything about having been present, or of a subscription having been got up for him. Was coming back, as he went without leave.

Mr Clarke.—Is this the same name—is it the man?

The coroner.—Oh yes, it's no more than what I expected. Mr Norminton (who was in court), have you seen the boy Goom since?

Mr Norminton.—No, Sir.

The coroner.—Ah, I'm afraid he has heard from his grandmother. (Laughter.)

John Goom, the hero of the letter, was sent for and was examined. He denied that the soldier had ever said a word about White, or that he had ever mentioned a word about the subscription. He had said that he had seen the flogging, and that he was going to give evidence at the George the Fourth Inn.

How, having been asked, denied that he had, or that he had ever said he had, seen the flogging.

The coroner.—There is no use in asking any more questions. There is clearly no truth in the letter.

John Darley, the house sergeant, who had been assaulted by White, and for which offence the unfortunate man was punished, here was sworn, and repeated the evidence, referring to the circumstances of the assault, which has already appeared. He had, he said, suffered severely from the effect of the blow, having remained for three days after being struck under medical treatment. He had had no previous quarrel with the deceased.

The coroner.—Did you ever notice anything odd in White's manner? —No, he was a man that knew himself very well, and, in fact, a superior and respectable man; by no means an habitual drunkard. He (Darley) had been present at the court-martial, and there White pleaded guilty. The assault was not provoked.

The coroner.—Well now, Mr Clarke, we are in a condition to read the depositions. The evidence, so far, is sufficient. It is quite clear that White did strike his superior officer, and that he was flogged for it; and now, when the depositions have been read, we can hear the medical gentlemen, and see what is the conclusion to which they have come,—what, in their opinion, was the cause of White's death.

A juryman.—I should like to know the way in which White was tied on the ladder, for I am told—

The coroner.—Say nothing about what you have been told. As it is the wish of the jury, the ladder shall be produced.

Sergeant Potter said he had sent for the deceased's account-book, and had it handed to Colonel Whyte. It was not a regimental book.

After some reluctance,

Mr Clarke produced the book, declaring that there was no wish to withhold it beyond a disinclination to produce a book which, as it related to regimental matters, the public had nothing to do with.

The coroner having examined the book, with the view of proving deceased's handwriting, returned it to Colonel Whyte.

Sergeant Alexander Wright was here called, and said—I know the deceased's handwriting, having seen him write, and I have in my hand a letter which I recognise as his handwriting. It is dated 25th June, Hounslow, and addressed to W. K. White, Post-Office, Leeds. The letter was here read, and is as follows:—

"Barracks, Hounslow, June 25.

"Dear Brother,—I am really ashamed to write after so long a silence, but conscience provoking me for neglect to you and mother compels me to say something, adding to the case of necessity of sending to you a letter to Tom, who wrote to me, and of course requires an answer, and how to send it bothers me, and most confoundedly. Be kind enough to despatch it when this arrives. He states to me that you received a letter before me, and I think you might have sent me more, but I suppose you thought to punish me for not writing. If the direction is not right please alter it for me. I am very unwell, and have gone through a great deal of trouble since you heard from me last, and feel disgusted with the service, but you need not tell mother so, for she would only fret. I hope you are all well by this time. Give my best love to sister and mother. I had formed a resolution to commit mischief, but thank God I have for the present altered my mind. I have not the confidence in myself I used to have. My star of destiny frowns at me, and everything goes wrong. Your letters give me a consolation generally, but I cannot venture to read them always, for I am very near approaching to lunacy at times. I am an old man now to what I was, and what a few more years will make of me I am afraid to guess at. . . . . write to me soon and give me a word how matters are in Leeds, for you are the only person I correspond with now. I have not seen London yet, nor shall I unless I change my mind—every one there is lost to me now. Again, I hope you will keep quiet to mother that I am ill, for her sake. Give my respects to Maclin, and Whitfield, and Kepple. You must excuse my faults, and forgive my failings, for I cannot help them; fate wills my proceedings, and I must go with the tide. My paper is short, and I must cut short my tale of woe, and resume my most horrid and wretched duty of a soldier. God be with you and prosper you, and in your prayers remember your affectionate brother,

"Frederick John White."

Sergeant Potter.—After being in the hospital on the 15th White got up from his bed on the 20th. I was not sworn when I stated before that he was in bed about a fortnight. He got up on the Saturday following his flogging. He was allowed to do so after he could wear his shirt, and to walk about the ward and assist in cleaning it. He then appeared to be perfectly well, with the exception of his back, and he never made any complaint. I can't say positively when he last cleaned the ward; but on Saturday, the 4th of July, he voluntarily washed it out, and Senior saw him do it. When it was fine he used to walk out in the fields. I have not been spoken to or complained of in any way since giving my evidence, but I have had more time to think over the matter, and I remember that he dressed Mathewson's back on the Monday.

The coroner remarked upon the strangeness of the witness's contradicting his former evidence.

Mr Clarke.—Your adjournment enables the witnesses to look back deliberately at the circumstance.

The coroner.—I hope it has been of advantage in many respects.

Witness.—The deceased scrubbed the floor, and then mopped it, and was occupied in doing so for 20 minutes or half an hour. I was not told by the surgeon that it was dangerous for the deceased to go out, and White made no complaint to me till the 5th of July, I being the person to whom such complaint ought to be made. I did not notice at the time any particular change in the weather which would make it necessary to warn the deceased against going out.

The coroner here made some inquiries as to the mode and time the entries were made in the medical book.

Dr Warren explained that when the case was an acute one the entry was made each day, and when it was a chronic twice a-week. He considered the deceased case acute from the 5th or 6th of July.

The coroner.—The difficulty is about when the entry on the 4th was made. That is a very important point. Sergeant Potter, can you say positively when the entry on the 4th was made?

Sergeant Potter.—I have no doubt but that it was made on the 4th.

The coroner.—When did you first see the entry, for if you are in the habit of reading such entries this one must have struck you.

Sergeant Potter.—The entry was made on the 4th to the best of my belief. Indeed I am positive it was then made.

The coroner.—How is it Mathewson's case was not entered in that book, Dr Warren?

Dr Warren.—I did not put down Mathewson's case, because it was so slight as not to be worth recording.

Sergeant Hooper, an invalid during the time of deceased's confinement, and discharged this day from hospital, corroborated the evidence of Sergeant Potter as the time when the deceased got out of bed. There was no occasion to stoop much at the cleaning out of the ward, which was very light, and occupied no more than five or ten minutes in the morning. White never said to me what the cause of his illness was, and I never asked. I did not hear him complain till the Saturday previous to his death. Invalids must leave the hospital on the doctor's order, but they return to light duty. After a flogging, some men who are fleshy, take longer to recover, but the general time is from ten days to three weeks. I never till this last time was in a hospital when a man was invalided by having been flogged, and until Mathewson's case I never saw the back of a man who had undergone that punishment.

Mr W. Hughes, second clerk at the judge-advocate general's office, and residing at Lansdowne-cottage, Lansdowne-place, South Lambeth, produced the whole proceedings connected with the court-martial, and the confirmation of the sentence, signed by Sir John Macdonald, adjutant-general. We select from the papers produced the following extracts:—

Members of the district court-martial which tried Frederick John White.

President—Captain A. Shirley.

Captain C. Hagart,	Captain C. Wyndham,
Captain T. Preston,	Lieutenant J. Hagart,
Lieutenant J. Hely,	Lieutenant Sir W. Russell.

The prisoner, Frederick White, No. 689, 7th Hussars, being put on his defence, hands into the court a written document, which was read to the court, and is as follows:—

"DEFENCE.

"Having pleaded guilty to the charge preferred against me, it only remains for me to urge some extenuating point by which I may become entitled to some clemency at your hands.

"I am sure that it cannot be said that the unfortunate affair was premeditated. I never had a word of any kind but such as should pass between a private and his superior officer, having always borne myself towards Sergeant Darley with that respect which was due to his rank; and I most deeply regret that the drink which I had taken deprived me of all control over myself, and obliterated from my memory those feelings of respect which had up to that time always influenced my conduct.

"I am well aware that an action committed under the influence of drink obtains but little consideration on that head, yet from the absence of all vindictive feeling on my part, added to the unconscious state I was in at the committal of the crime, I have to hope you will deal leniently with my case, and allow what I have urged to have some weight with you in coming to your decision.

I am, Gentlemen, your obedient servant,

FREDERICK WHITE.

"A. Shirley, captain and president, 7th Hussars."

"Approved and confirmed by order of the commander-in-chief."

Sparkes, who, it will be remembered, spoke as to the manner and the occasion of the flogging, corrected his evidence as it appeared in the deposition, and said that when, in answer to the question of, "Why were farriers selected, before other men, for flogging?" he replied that it was "because they were stronger men;" he had meant only, not that they intentionally struck harder, but that they gave heavier blows as a necessary consequence of their greater muscle.

S. Cook, who, on the first day of the inquest had corroborated Sparkes' statement, now further stated that on Friday morning last, when on parade for muster, Colonel Whyte addressed him and said, "Now, Mr Cook, I wish to speak to you with regard to your testimony at the late inquest." The colonel then informed him that he had made inquiries about what he (S. Cook) had said about trumpeters flogging in cavalry regiments, and that he had found it was not the case, upon which Adjutant Ireland remarked, "No, Sir, it never was." Colonel Whyte then said, "Oh, yes, I find that such a case took place 15 years ago." He (S. Cook) answered that the 1st Dragoon Guards flogged with trumpeters, and he believed other regiments did the same. Colonel Whyte denied it, and told him that he had "as near committed perjury as d—n it." The colonel then turned to the trumpeters and said to witness, "Do you call Luke Bust a boy, or Taylor?" and the adjutant said, "Or Yawl?" and he (Cook) then told the colonel that he had spoken generally, and had not referred to particular men. [Cook in his evidence spoke of trumpeters as striplings.] Colonel Whyte observed that it was clear there had been a misconception of the whole affair, and ended the conversation by saying that Cook had been wrong in the statement he had made as to the severity of the punishment in the 7th Hussars, as the fact was, there was less flogging in that than in any other regiment in the service. The men heard this colloquy. The colonel never threatened him (Cook) with punishment.

The coroner.—How do you know that there is severe punishment in your regiment?—I know it from report.

Colonel Whyte desired that Mathewson might be called, as he could explain the reason for the observations which he (Colonel Whyte) had made.

The coroner.—Oh, of course; I have just received a letter from the brother of the deceased man, requesting that the chaplain may be examined, and I think the application ought to be complied with.

Private Mathewson added, after reiterating portions of his former evidence, that Dr Warren never saw deceased or examined his chest when undressed. He (Mathewson) had been tried, sentenced, flogged, and sent to the hospital, all in five hours. Dr Warren saw him before he was flogged, but had merely felt his pulse, and asked him if he had any complaints. He had not been subjected to any punishment in consequence of the evidence he had given. The colonel had, in fact, released him two days before the term of confinement to which he had been sentenced had expired. On the Saturday before his death, deceased told Dr Warren, "I have a singular pain here," (pointing to his side); and Dr Warren answered that that would go off, and gave him some cooling medicine.

The coroner.—This relates to the day when Dr Warren states the deceased was well in general health, and would be ready to go out next day?

Witness.—The first Saturday after I went into hospital the deceased was going out, but there was a review, and the doctor would not let him return to duty because the pouch-belt would gall his back. It was, I think, on the next Wednesday, the 1st of July, that he first complained to me. On Saturday the 4th he told the doctor he had a singular pain at his chest, and it was then the doctor said it would go off, and ordered a cooling medicine.

At this point of the proceedings, it being 3 o'clock, the coroner granted an adjournment to half-past 4 o'clock.

When the jury again met at that hour, they went to the yard to examine the ladder, and see the position in which it was put. While there, and while the major farrier was explaining the position in which the deceased was placed on it, a question arose as to the possibility of the muscles of the back being moved while the culprit was under punishment.

The adjutant, who was standing apart, was called on by the jury to state whether the muscles of the back could be moved, and he stated his opinion that they could. This was denied by a juryman, who said in addition, "You have brought a pretty number of men to the ladder, I know."

On the return of the jury.

Colonel Whyte begged leave of the coroner to make a remark. He then repeated the preceding circumstances, adding, such an observation was not proper at such a time, or any other time, and showed an animus that he should not have believed could have found a place in the breast of any British juryman.

The coroner.—It certainly was an unfortunate remark, but the fact is, that the jury should have been allowed to see the ladder alone, and my order was to that effect. Was the ladder put in the right position?

A juryman.—Mathewson says it was not, and he showed me the way in which it was placed.

The coroner here directed the medical gentlemen present to go out and see the ladder, which they did. Then turning to Dr Warren and Mr Clarke he said, I find nothing in the authorities I have consulted about the handles of the lash, or the weight of it, or the number of knots. Are there any instructions given to the medical officer as to what he is to do on such occasions; for, if there are, they must be produced?

Dr Warren.—None. There are no such instructions in the British army.

The coroner.—The British army must be a queer sort of machine. (Laughter.) It is very extraordinary that there should be nothing for your guidance. But I must confess that, after looking at the law on the subject, I can find not a word. Nothing is said about the mode in which a man is to be examined who is to undergo a court-martial, whether he is to be stripped, or examined with the stethoscope.

Dr Warren.—I have done, as I believe every medical officer has done in such cases for the last 50 years in the British army.

Mr Clarke.—Do say yes or no, Dr Warren. I wish to God I could get you to say yes or no. (Laughter.) There is nothing to be guided by in military flogging but the customary rules of the service—*Consuetudo facit legem.*

Colonel Whyte.—The words of the articles of war are, "according to the custom of war in like cases."

The coroner.—I can find nothing to define that custom; for though the custom may be law, we don't know that it is.

Mr Clarke.—You shall be provided with proof of the custom.

The reading of the depositions having been resumed,

Private Ellsworth, on his former evidence being read over to him, gave the following additional particulars:—The sergeant did not fall when the deceased struck him with the poker, nor was the blow a severe one, but he ran away. I was recommended for promotion by my commanding officer, Colonel Whyte, at Ipswich. I told him that I was punished by a fatigue extra beyond any other man in the regiment. I had been tried by a district court-martial, but appealed to a general court-martial, and was told that if I gave up the application for the general court-martial, the proceedings of the district court-martial would be annulled; but if I did not the sergeant's wife would be brought in evidence against me. At Ipswich other men besides me saw my name in the office on a card with a recommendation for promotion.

The coroner.—Were you ever flogged, Ellsworth?

Witness.—I was flogged once by order of the sergeant-major. It is called "boating," and I was punished in that manner, though a young soldier just joined, and a disgraceful thing it was. I was ordered by the sergeant-major to get five dozen with a shadrock strap. It was doubled up and used with the buckle end, while I was placed on the table and struck with it. Owing to my having a good character four dozen were forgiven, and I had only one dozen. I could not look up after that, and I deserted the regiment, but being advised by my friends I came back again. I was ashamed to see myself walk about, and I could not rest where I was. I was ill while I was away. I never knew this sort of punishment inflicted on any other man but myself since I have been in the regiment.

The coroner.—Why were you punished?—I was cook of my room, and I went out to get the meat for a pudding. I did not, however, bring the suet, and as the pudding could not be made without it, I was reported by the other men to the sergeant-major, who said, "Take and give him a dozen." (Laughter.)

Private Evans, in addition to his previous evidence, when the depositions were read over, stated, I understood that I was to do my duty in a lenient manner. I had practised the mode of flogging previously with the farrier-major.

The coroner.—Not on the farrier-major. (Laughter.) How did you know what to do?

Witness.—I understood it from the adjutant, and I had practised on a sack. (Laughter.)

The coroner.—How did you practise on the sack?

Witness.—I was directed to strike it in the centre, but I was not told to hit hard. In practising on the sack I as far as possible tried not to let the lashes vary or spread from the same point.

Farrier-major Critten, at the request of the jury, was called and examined—He had, on the 15th June, assisted in the flogging of White. He had received no orders before arriving at the riding-school. He had been told, after the sentence was known, by the adjutant, to "get ready" with the cats. He had himself made the cats, and had brought them from the veterinary surgery. Had received no instructions as to the mode of flogging. He had been taught how to flog by the farrier-major to whom he succeeded in the 7th Hussars. There were no printed or written instructions for farriers as to the mode of flogging. (The cats were here produced, and Critten singled out the particular one with which he had commenced and finished the flogging. Some of the cats (it was shown by the coroner) had double knots; some seven, some eight, and some six knots.

The coroner.—What are you told when buying the whip-cord?—To get cord as nearly as possible like the cord used before.

Adjutant Ireland.—The cords sometimes get untied, and then, by chance, there may be a double knot.

The coroner.—But is there no law?

Adjutant Ireland.—No, none. This is called a cat-o'-nine tails, and that means, of course, nine knots.

The coroner.—But was there never any law about it?

The adjutant.—I don't know. I am not bound to know the law—only to carry it into effect.

Mr Clarke.—We show you the practical; we will by and by—

The coroner.—Oh, I hope not. (Great laughter.)

After some further explanation, or rather want of explanation, on the part of the adjutant,

The coroner said, that he really thought, if such an instrument were used, it should be used according to defined and known rules.

Critten said that he never doubled a knot. When a knot did become double he cut it off.

The coroner.—But here, on one cat, seven out of eight knots are doubled.

Adjutant Ireland was ready to swear that the cat with which White had been punished had been single-knotted only.

A juror.—This double knotting is clearly not the result of accident.

Critten would positively swear that the double knots had not been made by him, and that no one else had made them.

The coroner.—Well, but how did they come here?—He did not know, only he was sure they were never made by any man. (Laughter.)

The evidence was then resumed.—He had given no orders to Evans. White had been flogged just in the same way as other men were flogged. Had been fourteen years in the regiment, and had never, when called upon to flog a man, received orders. He listened to the sentence, and then went to flog the man sentenced. Had seen old farriers flog, and as well as he could he imitated them. Never had any direction as to the weight or the force of the blow. Struck as he thought best. Had never seen the doctors interfere at a flogging.

The coroner.—Do you know, or suppose you know, from the writhing of a man that he is suffering?—I cannot answer that. White stood his flogging very well. Once, when struck on the neck, he cried out, "lower." The colonel, adjutant, and doctor, had always been present at the flogging in the 7th Hussars. He had seen other men who had been flogged suffer apparently more than White, and had no enmity with White. Evans was very good friends with White. Both he and Evans had spoken to White in a friendly way before his death. Had seen men sometimes in their struggling, pull down the ladder to which they were tied.

The coroner.—Then you flog as you think best?—Yes, Sir.

The coroner.—You have no instructions and never had, from the adjutant, how to strike, where to strike, or with what force to strike?—No, Sir.

The coroner.—Did you see any man faint?—I don't know, Sir. I didn't see.

The coroner.—You were otherwise employed?—I were. (Great laughter.)

The coroner.—How many men have you flogged?—Don't know.

The coroner.—Will you swear you have not flogged 20?—No, Sir.

The coroner.—Thirty?—Yes, Sir; I have not flogged 30.

A juror.—How many have you flogged in the last four years?—Three.

The coroner.—Do you rise on your toes when you strike?—It's a palpable falsehood; I never did so in my life.

The coroner.—You don't strike with all your force?—No, Sir; I give a fair blow. We flog quicker than in any other regiment.

Dr Warren supplied an omission in his former evidence, when he neglected to say that the left lung was slightly inflamed.

Some of the other depositions were then gone through, and

The Rev. Dr Joseph Benson was examined and stated—I reside in Hounslow, and am chaplain to the barracks. Previous to his death, and on the day he died I saw the deceased. I read the prayers in the hospital every week, and he was present, though I was not aware of the punishment he had undergone. I attempted to converse with him when I saw him, but he was then sinking. He said in reply to my questions. that he got ill on the Wednesday, and that he had since been getting worse and worse. He said nothing to me about the flogging, and he soon became insensible and remained motionless. The numbers who attend for prayers at the hospital vary from 10 or 12 to 16 or 17. The deceased did not speak to me, and made no complaint when I was in attendance to read prayers.

The coroner.—In an inquiry of this kind, gentlemen of the jury, I feel there should be no secret between us, and I have now to inform you that after the adjourned inquest I asked his brother whether the deceased White was in a generally correct state of mind, and, in fact, if there was any insanity in the family. The brother then put into my hands the letter which is now in evidence, and I have since received from him the following communication:—

"*Leeds, August* 1.

"Sir,—When you questioned me after the adjourned inquest on Monday last, as to the sanity of my brother, I replied that I had often had fears for the state of his mind, and grounded my opinion on these reasons:—When a boy at school, and about 13 years of age, he suffered severely from aberration of intellect, caused (as it was said) by fright occasioned by a fall from a wall. For some months he was excessively weak in his mind, indeed insane. Then on several occasions he has displayed towards members of his family such extraordinary conduct as could proceed from none other than a deranged person. In truth the very fact of his enlistment in the Hussars so soon after his discharge from the Guards, coupled with his repeatedly expressed dislike of a military life, proves anything but perfect sanity. In the early part of this year I wrote to him, informing him of his mother's serious illness; and although he had at all times before shown the greatest love for her, he never acknowledged the letter, or made inquiry respecting her. His letters (I ought to state) all partake of the same excited feeling which you may perceive pervades the one that I left with you at Hounslow. When sane, I do not think a more generous, affectionate, fascinating man could exist than my poor brother; but when irritated, or intoxicated, he was most dangerous. Indeed, we were at all times very careful in our conversations and correspondence to avoid exciting his feelings. He was exceedingly impatient of control. It is also singular that his mother (who is ignorant of the particulars of his dreadful suffering), when informed that he was ill, exclaimed that 'she feared he was insane, and prayed that he might not be so.'

"My late father, owing to several family and other afflictions, for the last few years of his life, was decidedly incorrect in his intellect, and was compelled to surrender his official appointment; and the behaviour of a late brother has at times partaken of an eccentric feeling, not to say more of it.

"I ought to state that I know very little of my brother's condition during his stay in the army. He wrote then very seldom, and appeared at all times quite tired of a soldier's life. What I have stated occurred prior to his leaving home; but I have now no reason for thinking that the violence of his temper or the wild state of his conduct have abated since.

"I had not seen him for a year and a half before his death, when he spent three weeks here, and became a general favourite. I thought his conduct much improved at that time, and that he was at last becoming alive to his position, and that he was become a man quite competent for promotion; but it has pleased Providence to order events otherwise.

"If it be not irregular, will you be good enough to ascertain of the chaplain if he attended my brother, how often, and whether my brother was aware of his condition? I think that when a man's mind is prostrated by the degradation and suffering consequent upon punishment, the consolations of religion would, I should think, have been most acceptable; and reflections on the agony of our Redeemer, for no crime whatever, were calculated to do much towards making a better man.

"Convinced that you will see justice done, I have the honour to be,

"Sir, your very obedient servant,

"WILLIAM KEPPLE WHITE.

"To T. Wakley, Esq., M.P., George the Fourth,
Hounslow Heath, Hounslow."

Colonel Whyte here expressed a wish to make an explanation which would throw light upon the evidence of Cook, and also, perhaps, enable the jury to judge of the credibility of some of the witnesses adduced before them. He said,—On Wednesday, before the parade for muster, when Cook stated that I had him out and spoke to him in front of the regiment, I had had Mathewson in the office, and I there asked him whether the coroner had not inquired if he had been punished for anything he had stated. His answer was that the coroner had. I then said, "Now, Mathewson, I beg you will dismiss from your mind that I will now, or hereafter, punish any man for his evidence given at the inquest, or will ever refer to it in any way." I went on to say, "you are a smart intelligent soldier, capable of anything. If you conduct yourself well you will find me your friend, and if not I will punish you, as I would any other man. Your future station in the regiment depends entirely on your future conduct, for I won't revert to this." I then inquired if he was under punishment, and, finding he was, had it removed. I added, "I mean this for you and all the other witnesses now before the court. The following morning, the adjutant came to me, and said there was the greatest excitement in the regiment in consequence of the preceding conversation. That when Mathewson left the room he mentioned to Senior I had brought him into the office, handed him a chair, and asked him to sit down by me; that I told him I would make him a corporal immediately, and that during the time he was with me the adjutant came to the door and knocked, and I asked him if he had any objection to my admitting the adjutant. The adjutant said that there was the greatest excitement on the matter, that the non-commissioned officers thought themselves aggrieved, and the privates thought it was compromising the matter, and that he himself felt his credit at stake. Under these circumstances I felt it would not do to speak on the subject privately to Cook, and that the matter should be spoken of publicly at parade. The dangerous part of the thing was, that the men thought I was compromising the thing; and I think it necessary to make this explanation, in order to show why I took a step so extraordinary as speaking to Cook on parade.

Private Mathewson.—I say, Sir, the colonel did tell me he would make me a corporal, and handed me a chair, and asked me to sit down. When I came in he ordered every one out, and Sergeant Wright also went out.

Colonel Whyte.—Sergeant Wright had better be sent for.

The coroner.—I have been very willing to hear what you had to say, for your conduct towards Cook I considered improper without explanation. In all these inquiries, when incidentally anything comes out prejudicial to character, I think it only fair that an ample opportunity of explanation should be given.

Sergeant Wright.—I was present during a statement made by Colonel Whyte to Mathewson, I think on Tuesday. Private Mathewson was standing, and no expression was used by Colonel Whyte which could bear the construction that Mathewson was to be promoted. I could not have been absent more than a minute or two, if I was absent, but I am positive I was not.

Private Mathewson.—The colonel has taken advantage of my position. I have, of course, no witnesses, for no privates of my troop were present; but when I next am called on to speak to my commanding officer, I shall take care that I apply to have some of my comrades present.

Sergeant Wright.—I am on my oath, and I am as incapable of perjury as any man here. I repeat my statement.

Some further conversation on this point took place, in the course of which Mathewson resolutely stuck to his assertions, which were as resolutely denied.

Private Senior was then called, and stated that Mathewson had communicated the above facts to him, which however the latter stoutly denied. A very amusing effect was produced by the efforts which Senior, who was rather intoxicated, made to appear steady and sober.

Colonel Whyte.—If I am allowed to prove it, I will show that almost every single word stated by Ellsworth is quite untrue. As an instance, I think he said that he had been punished seven times. Now here is his defaulter's list, in which his name is 32 times mentioned.

The punishing system does not seem to succeed, Colonel. (Laughter.)

Colonel Whyte.—You can try any different means with men like Ellsworth, and see how you get on. He has been three times tried by courts-martial besides his "cobbings."

J. Parry, regimental quarter-master in the 7th Hussars for five years, and upwards of 32 years a soldier in the same regiment, stated, that during that time the mode of punishment in the regiment was by farriers, as at present.

The depositions of the medical witnesses were here read to them.

Mr Day made a complaint that Mr Wilson had, in addition to the statements they had agreed on making at the *post mortem* examination, given in evidence opinions which he (Mr Day) had not acceded to, and from which he dissented. He did not consider that inflammation of the pleura could be caused by the muscular disorganization observed, for that was covered by the intercostal muscles.

The coroner.—But is it not a fact, Mr Day, that a formation of mattery substance connected with the veins of the skin is often the cause of inflammation of the lungs?

Mr Day.—Yes, it is so; but I do not think that in the case of the deceased, the muscular disorganization was the cause of death.

When the reading of the depositions was concluded, the medical gentlemen were ordered to leave the room, and each was summoned in separately in the order in which he had given his evidence, to say what, in his opinion, after having heard all the evidence in the case, had been the cause of deceased's death.

Dr Warren, the first in order who entered the room, repeated his former statement, that White had died of inflammation of the pleura and heart, with its connecting vessels, and that the flogging had in no way caused his death. The deceased was not depressed in spirits, for had he been so, he, Dr Warren, who visited the hospital twice every day, must have noticed the fact.

The coroner.—Is it your belief that if he had not been flogged, he would have been alive now?

Dr Warren.—That is a question which no human being can answer; and the man who ventures to do so puts a great deal at stake.

The coroner.—You cannot fail to bear in mind that on you devolved the duty of examining the deceased on the day he was tried, and again on that of his punishment. The jury will therefore infer that you satisfied yourself he was a sound man.

Dr Warren.—I find, that in the admission and discharge-book, he has been only three times in hospital for six years. Had I found him faint during the flogging, I would have ordered him to be taken down. I admit that an intermitting pulse shows a greater state of danger than even fainting; but I have no instructions on such occasions as to how I am to discharge my duty—(A pause here followed, after which Dr Warren, with a degree of feeling which excited marks of applause in court, said)—I beg to say that it is to me a most painful duty to witness such an operation, and I should be most happy to think that I should never be called upon again to witness so disgusting a scene.

Dr Hall, the next medical gentleman admitted, said,—after hearing the evidence I am still of opinion that the deceased died from inflammation of the heart, lungs, and pleura, and notwithstanding the evidence of Mathewson and Reilly, I cannot connect the disease with the punishment, or subscribe to the novel doctrine of Mr Wilson. He does not ascribe this changed condition of the deep-seated muscles to the direct violence of the lash, as they were protected from that by their position, and by the superjacent parts, but to excessive contraction during the agony of punishment, and subsequent disorganizing inflammation from nervous depression. Now, White was in the prime of life, and does not appear, by the statements we have just heard read, to have suffered much from nervous depression. It is strange, therefore, and unaccountable to me, why nature should have made no efforts to repair this injury, or that her efforts should have been of a disorganizing kind in this particular spot, while the process of healthy reparation was proceeding rapidly in other parts of his frame. The injury itself, too, is one of a novel kind, and difficult to reconcile to our preconceived opinions of the limited power and action of the small muscles alluded to. White, the witnesses all say, bore his punishment without any struggling, and Dr Warren says he observed no spasmodic action of the muscles of the spine. Besides, had Mr Wilson ever witnessed the infliction of corporal punishment, he would have seen that it is the superficial layers of muscles which attach the shoulder-blades to the spine that are thrown into action, and had any rupture of muscular fibre taken place from violent contraction, it would most likely have occurred amongst those, and not among the deeper-seated ones along the spine, which have such limited contractile power, and are so well protected from injury. Mr Wilson lays much stress on the injurious effects likely to have been produced by this altered portion of muscular fibre lying in contact with the intercostal spaces; but it seems to have escaped his recollection that the cavity of the chest on that side was filled with diseased and disorganized viscera, and highly putrid fluids, which were much more likely to have contaminated the adjacent muscles, than that the small portion of pulpy fibre should have occasioned the extensive disease that was discovered within the chest. The only wonder is, after eleven days' exposure of the body, at this hot season of the year, that the change of structure was not still more extensive. Mr Wilson states that he has dissected from 500 to 1000 bodies in the course of his professional researches. I have also served much in warm climates, where deaths unfortunately are very common, and decomposition of the body after death very rapid, and I should say from Mr Wilson's description that the change he observed was merely the first stage of putrefaction.

The coroner.—Is it not the limited powers of contraction in the muscles which Mr Wilson refers to which makes them subject to injury?

Dr Hall.—I should say that the change observed by him was the first stage of putrefaction. I regret that I was excluded from the subsequent *post mortem* examination, and I enter my protest against that exclusion.

The coroner.—Will you state what right you had to be present more than any other person?

Dr Hall.—I think it was only fair from the particular turn which public opinion had taken on the subject that I should be present.

The coroner.—In point of fact, the irregularities which have preceded have brought us into many difficulties.

Dr Hall.—I don't admit that those proceedings were irregular, because, in point of fact, it was a public examination.

The coroner.—How happens it that you were sent for at all, Dr Hall? What were the special circumstances which led to that? Do you not believe that it was in consequence of the deceased dying so soon after his being flogged?

Dr Hall.—I was sent to make the *post mortem* examination in consequence of the report I had previously made about White to Sir James M'Grigor.

The coroner.—In your report you say, it appears White received corporal punishment of 150 lashes. Why did you mention that fact, if you did not connect it with his illness?

Dr Hall.—I was bound to say all I knew, and I considered it important to state that fact.

The coroner.—It would have relieved us of a great deal of difficulty if notice had been given at once to the constable, and an inquest had been constituted on the first *post mortem* examination. You have made your report, and sent in your opinion, and now you complain that you were not present at an examination made by a surgeon who came before the court as an unbiassed person. You yourself have in your report connected the flogging with the death of the deceased.

Dr Hall.—The connection exists, but it is not one of cause and effect.

The coroner.—Could you form any idea as to the state of deceased's mind from the *post mortem* examination?

Dr Hall.—No, I could not; the appearances of disease in the brain were not sufficiently decided for that; but they looked as if the deceased had had *delirium tremens.*—Dr Hall concluded by repeating his belief that the death of White was to be accounted for by the sudden change of temperature on the 5th and 6th of July, amounting to a difference of more than twenty degrees.

The coroner drew Dr Hall's attention more than once to the expression used in his report, that the circumstances of the case were peculiarly interesting.

Dr Reid and Mr Day repeated their belief, expressed in evidence on a previous day, that the deceased had died from inflammation of the heart, lungs, and pleura, brought on by atmospheric changes, and having nothing to do with the punishment inflicted on him. The punishment might have had some effect in producing the disease.

The coroner here asked Dr Hall if he considered the letter, dated the 25th of June, written by deceased to his brother, to be the letter of a man in cheerful spirits?

Dr Hall replied that there appeared only one passage in this letter which could indicate to the contrary, and, in opposition to the conclusion to be drawn from this, he set the statements of all the witnesses, that White had not been, and had not appeared, depressed in spirits.

Mr Wilson was then called.

The coroner put the question.—After hearing the depositions, what do you consider was the cause of the deceased, White's death?—No question of it, the flogging, and its consequences upon his health. The inducement to me to form that opinion is, that death from flogging is of no uncommon occurrence. The diseases following upon flogging in general are identical with those which manifested themselves in the case of White. I hold in my hand some abstract cases of flogging, and these will prove that my opinion is confirmed by the greatest men in the country.

Dr Hall was here addressed by the coroner, and was asked if he had any opposing authorities to refer to.—He had not. He had seen several hundreds of cases of flogging, and death ensued in one case only out of every thousand.

Mr Wilson resumed.—Extensive injuries to the skin will produce serious internal irritation. Thus a burn will give rise to fatal disease of the internal organs, viz., stomach and bowels, heart and lungs, liver and kidneys, so that prolonged irritation of the skin in this case was calculated to produce disease. Then there might have been secondary disease, inflammation of the heart and lungs, weeks after the application of the cause of the original disease. Such cases are frequently to be met with in our hospitals; so that whether I look at the primary or secondary effects of flogging, I see in both a cause of fatal disease. Heretofore it has been considered that the injuries resulting from flogging are confined to the skin, but I have given evidence that in this case the flogging was followed by a pulpy, softening and deranged state of the muscles.

The coroner.—You will remember that there was in this case a partial recovery.

Mr Wilson.—It is not an uncommon thing that when, after flogging, the patient is pronounced to be, and thinks himself to be, out of all danger, internal inflammation then commences and that results in death.

The coroner.—Leaving out of the question mere speculation, is it your decided opinion, from all you have heard of the history of the case, that the death of White was caused by flogging?—I have not the slightest doubt that it was.

The coroner.—After deliberating upon all the facts, heard in evidence or observed by yourself, and deliberating upon them as cautiously as you can, do you entertain a doubt on the subject?—for if you do, it is your bounden duty to state it.—I have not the slightest doubt on the subject.

Some conversation having ensued between the coroner and Mr Wilson, touching the contradiction which Mr Day had offered to the testimony of the latter,

The coroner said, you have opened from 500 to 1000 bodies, and in cases of these examinations did you find such a state of the muscles, as that perceived in the case of White, to result from decomposition?—No, in no one case.

The coroner then proceeded to address the jury and to sum up the evidence. The case has already occupied in its investigation so long a time, and so many irrelevant matters had been brought before them, that he, before touching on any one fact of the case, must entreat of them to dismiss from their minds all that had been said, and all that had been written, and all that had been reported or rumoured, and apply themselves solely and only to the matter which was before them. If ever there was a case which, more than any other, required a jury to be absolved from all prejudice and freed from all mere impressions, this was the particular case in which the mind should be so disciplined. The inquiry had from the first been surrounded with innumerable irregularities; these irregularities had almost necessitated further irregularities; and he trusted that what had passed would be accepted as a lesson by those persons who had desired to avoid that investigation which the law demanded—that it would teach them, whatever were those contrivances, the law in the end would prove too strong for the success of their object. In this case there had been a deliberate attempt to hold back that investigation due to the law, to the regiment, and to society, and, above all, to the relatives of the deceased. The coroner then went on to detail the circumstances—the refusal of the Rev. Mr Trimmer to bury the deceased without the warrant of the coroner, and the suspicions which were rife in the neighbourhood and in the regiment as to the cause of death—which were the occasion for the calling of the inquest. When he (the coroner), on receiving information from Mr Trimmer, sent the constable to Dr Warren and to others connected with the barracks. Dr Warren stated, that White had died of natural disease springing from natural causes, having nothing to do with the flogging; and by others the number of lashes and the nature of the punishment had been greatly misrepresented. He had no doubt that Dr Warren believed in the truth of the opinions which he declared; but, how he could come to the conclusion announced, after what, according to the evidence, he had stated and admitted to White when in the hospital, was, it must be owned, a matter of the greatest astonishment. There had been further concealment on the part of the people at the barracks, in applying to the district registrar's office; for, at first, nothing at all about the flogging was mentioned: and this mystification had proved most unfortunate. If there was nothing wrong, there was nothing to fear; if there was anything wrong, investigation would be compelled by the law; and, for this reason, the whole conduct of the military authorities must be characterized as foolish. Upon the first view of the body by the jury, they had noticed that a large piece of skin had been removed from the back; this observation gave rise to various reports most prejudicial to the character of the officers of the regiment, and which, in reality, were not founded in truth. It had since been distinctly proved that so far from the skin having been removed as it had been said during the life of the man, it was not taken away till after the *post-mortem* examination had been made; and it was then taken away, not for any bad purpose, but to ascertain what was the condition of the back after the individual had died from having received such a punishment. Therefore had this irregularity of which he had spoken in the preliminary proceedings been avoided—had all been open, fair, and public, no one of these mysterious reports or these suspicious rumours would have gone abroad. Upon their first view of the body they were informed that an examination of the body had already been made by two staff surgeons acting in conjunction with Dr Warren, that a report drawn up by them had been made to the Director-General of Medical Hospitals: and so far there had been such an investigation that a distinct conclusion had been arrived at as to the cause of death. The functions of the coroner and jury, who ought to have been called in, had been anticipated, and to such an extent had this gone, that while afterwards the coroner and jury were endeavouring to form some idea as to the cause of death, it was being announced as from authority in the House of Commons, that the circumstances attending the flogging were altogether unequivocal, and that the death of White had no possible connection with the punishment inflicted on him. These were some of the difficulties which attend on the investigation, and prejudices too, of a most extraordinary character, had been created. Expectations as to the result had been expressed, and the jury therefore could not too carefully limit themselves in their inquiry to the legitimate evidence which had been produced before them. The inquest had been called, not because there had been a sudden death, or because there had been a suspicion of poison; the warrant had been demanded on the undisguised suspicion that White had lost his life solely in consequence of the violence of the punishment. The two staff surgeons having made their examination, and reported their opinions that the death resulted from natural causes, it became necessary for him to intimate to the jury to name their own surgeon to complete what had been only a partial examination. Mr Day had been named. Mr Day, however, himself made but a partial examination, and then confirmed the already expressed opinions. It had been thought necessary by the jury that a gentleman should be called from London, and that the body now buried some days should be by him examined. He had not hesitated in looking for the most competent person to fix on—Mr Erasmus Wilson. It was right he should say that Mr Wilson was a gentleman of distinguished acquirements in his profession, a first rate anatomist, and a gentleman with a high character for unimpeachable veracity. The jury, therefore, had to choose their own authority, to select between Mr Wilson and the three others, also eminent medical gentlemen. The law of the case was this: that the punishment of White was a legal punishment, and if they came to a conclusion that White had died in consequence of the bodily injuries following on that punishment, the verdict which they would return could not inculpate those authorities carrying into execution the legal punishment. The learned coroner then proceeded to dwell on the fact of the regimental surgeon having pronounced the deceased on being sentenced to be in a fit state to undergo corporal punishment, and of the absurdity of any medical man venturing to give such an opinion when the punishment was of that fearful character adopted by the army. He (the coroner) then at great length followed the evidence, medical and non-medical, and which spoke of the demeanour and of the symptoms when in hospital, and up to the day of his death exhibited by the deceased; and, after pointing out in further detail the exact bearing of the law regulating military tribunals, and the sentences they were competent to pronounce, he concluded by begging of the jury to remember, that if their decision was that death had resulted from this dreadful punishment, they were not called upon to fix any particular stigma to the unfortunate individuals who had taken the authoritative part; they were to leave to public opinion to stigmatize that odious, that monstrous, that abominable law which gave them the power, from the exercise of which such melancholy consequences had resulted.

A quarter to one o'clock on Tuesday morning. The jury then retired. They deliberated for about half an hour, and returning at the end of that time, the foreman read the following, as the unanimous verdict of the jury:—

"The jury do say that Frederick John White died on the 11th of July 1846, from the mortal effects of the severe and cruel flogging which he received on the 15th day of June, in the cavalry barracks, Hounslow; that the same flogging was inflicted on him under sentence passed by a district court-martial composed of officers of the regiment of the 7th Hussars, and duly constituted for the trial of the said Frederick John White, &c. That the flogging was so inflicted upon the back and neck, and that James Low Warren, the surgeon, and Colonel Whyte, the commanding officer of the same regiment, were present at the flogging by which the death of White was caused. And on returning the verdict the jury cannot refrain from expressing their horror and disgust at any law among the statutes or regulations of this realm which permits the revolting punishment of flogging to be inflicted on British soldiers; and at the same time the jury would call upon every man in this kingdom to join heart and hand in sending petitions to the British Legislature, praying for the abolition of every law and regulation which permits the degrading practice of flogging to remain one moment a slur on the humanity and fair name of the people of this country."

After a pause, Mr Wakley said he heartily concurred in the verdict. He had felt it his duty to make some inquiries of the deceased's brothers of his (the deceased's) state of mind, and the information he then received that day, strengthened him in the opinion that the deceased was in an insane state of mind at the time he was punished.

The proceedings terminated at 20 minutes to 1 o'clock.

The jury then separated at 1 o'clock on Tuesday morning.

Outside the inquest room, during the retirement of the jury, the utmost excitement prevailed, even to the time the verdict was returned. The inquest room was filled for a period of 15 hours, during which the inquiry lasted.

THE SOLDIER F. WHITE.—A letter in the *Northampton Mercury* supplies the following facts relative to the late unfortunate soldier F. White, of the 7th Hussars, whose father was postmaster of Nottingham. After leaving Mr Roper's academy, of Nottingham, and where I ought to state he met with sons of the most respectable families of the town, and one who is now an able barrister, he was apprenticed to Messrs Barker and Adams, large wholesale hosiers. I believe he continued with them about two or three years; and about the same time that his respected parents had to give up all that they possessed, the above house either stopped, or they decreased their hands; poor White was then out of employment, and he tried in vain, in Nottingham, to obtain a situation. He went to London, there met with disappointment after disappointment, till he was driven to enlist in the 2d Life Guards. He did not remain long in this regiment. He met with an accident—put his arm or his shoulder out—and was discharged as being unfit for service. After this he was in the London police, and was one of that body who was sent to Birmingham to quell the riots there in 1840. At this town he tells me in his letters that he enlisted into the 7th Hussars, being in liquor at the time. This account he gave me when at York Barracks. Poor White was a tall, rather slender young man; and I remember, when he was 17 years of age, that he stood nearly six feet high. He was of a kind, affable disposition, gentlemanly in his deportment, and respected by all who knew him.

The evidence given in the foregoing coroner's inquest is here recorded as accurately as possible, as it was taken in consequence of the importance of the medical questions which it involves, and the influence which similar occurrences have had and may have on the position of medical officers of the army. Whatever observations the inquiry may suggest shall be given in the review department.

PART II.

CRITICAL ANALYSIS.

Art. I.—*The Vital Statistics of Glasgow for* 1843 *and* 1844. Drawn up by appointment and under the authority of the Lord Provost, Magistrates, and Town-Council. By Alexander Watt, LL. D., City Statist, Secretary of the Statistical Section of the Glasgow Philosophical Society, &c. &c. Glasgow, 1846. Pp. 128.

The importance of, and, we may say, the necessity for, statistical evidence to confirm the truth of every branch of knowledge is daily becoming more and more generally admitted. Theories and hypotheses, the premises of which do not stand the test of numerical analysis, are now, we think, received with much greater reserve than formerly, and conclusions and deductions are more cautiously drawn from the limited experience and speculations of individual observers.

The many important facts which the study of statistics has brought to light, and the numerous errors which have been corrected by means of it, demand for it a high place in the estimation of every one at all interested in the advancement of science. To the statesman and politician it is of the first consequence, as they must be guided almost entirely by the results which they obtain from this source. The truth of this remark has been well illustrated in the all-engrossing subjects which have so recently been engaging the attention of the country, where the whole matter may be said to have resolved itself into one of pure statistical inquiry; every argument, whether for or against the government measures, requiring to be supported by a mass of strong evidence of this nature in its favour.

The study of vital statistics has for its object the discovery of those laws which regulate the condition of the people; and its advantages in the political economy and hygienics of a nation have been amply proved. Medicine, with which we have more immediately to do, has derived from the careful study of statistics more important services than perhaps any other branch to which statistical researches have been applied, and the facts elicited by their application to almost every department of the science, have contributed in a very great degree to its advancement. Our knowledge of the laws of mortality of different diseases has, in many instances, been greatly improved, and in some pretty accurately determined; while the study of physiology, pathology, and therapeutics has been much facilitated by being subjected to the scrutiny of the numerical method. Life assurance associations, and societies also, owe their existence entirely to the perfection to which this science has been brought; and the great accuracy with which their life-tables are constructed need not here be insisted upon.

We are, however, inclined to believe that much remains as yet undiscovered, which can be brought to light only by the careful and progressive improvement, which, it is hoped, will take place in the study of vital statistics. We are thus urgent in pressing this subject on the attention of our readers, in order to enlist on our side, all who at present, or who may at any future period, find themselves in situations where they may have opportunities of contributing to the advancement of this all-important subject, being fully convinced that it affords the only sure groundwork for correct deduction and generalization, and the only firm basis on which to establish the truth.

The vital statistics of a city of such dimensions and population as those of Glasgow, and whose inhabitants are subject to perhaps more vicissitudes than those of any other town in the empire, must be extremely interesting; and if carried on for a series of years they become of great value. The attention which has been paid to them, and the encouragement which those who have interested themselves in their advancement, have met with, are highly creditable to the citizens and municipal authorities of Glasgow; and among those who have distinguished themselves there, in this important branch of science, the names of Cleland, Watt, and Cowan, must ever be mentioned with honour.

Bills of mortality for the city and suburbs of Glasgow have been published since the year 1821, when they were commenced by Dr Cleland, who published one annually till the year 1835. After that period they were drawn up by Mr Henry Paul, and Dr Alexander Watt, the author of the present Report, both of whom paid great attention to the subject, and have, from time to time, added much very useful information to the science of vital statistics. The accuracy of the mortality bills for the last twenty-three years, which embraces a period during which the government census has been taken three times, may therefore be fully relied on. Before that period, however, the registers were in such a defective state that little or no confidence can be placed in the returns, although records have been kept since the year 1694.

The work before us comprises the statistics of Glasgow for the years 1843 and 1844, drawn up by Dr Alexander Watt, whose labours in the cause of statistical research merit our highest approbation. Since the duty of compiling these important documents devolved upon him, he has contributed many important facts to our knowledge of this subject; and his untiring zeal and industry have added numerous interesting tables to those formerly published along with the Glasgow bills of mortality, justly entitling his treatise to be called the "Vital Statistics of Glasgow," containing, as it does, a mass of very important evidence on the sanatory condition of the inhabitants of that city and its suburbs.

Our observations on the first or tabular part of the work are necessarily confined to a mere statement of the arrangement adopted, as we must subsequently follow the author in his deductions. We therefore reserve our remarks until we have given a sketch of the tabular arrangement.

First is given an abstract of the Glasgow mortality bills for the years already specified; showing the number of burials, arranged according to the districts and burying places; also an abstract of the number of births and baptisms, as recorded in the registers; these being still farther analyzed so as to exhibit the monthly returns. A similar arrangement and tabular view is then given of the marriages which occurred during the years 1843 and 1844.

Next follow the monthly tables of mortality, showing the number of deaths that have occurred at each age, from under one year to above a hundred, divided into eighteen periods of life; the diseases, &c. by which the deaths were occasioned being arranged as follows:—

CLASSIFICATION OF DISEASES.

Classes.	Species.	Synonymes.
Accidents.		Fractures, wounds, burns, bruises, drowning.
Aged.	Decline above 60 years and without any particular disease.	
Asthma.		Shortness of breath.
Bowel complaints.	Dysentery.	Flux, bloody flux.
	Bowel hives.	Inflammation of bowels in children.
	Looseness.	Diarrhœa.
	Teething.	
	Colic.	Iliac passion.
	Cholera.	
Catarrh.		Cold, influenza.
Child-birth.		Puerperal fever.
Croup.		Stopping.
Decline.	Consumption.	Pulmonary phthisis, wasting.
	Tabes mesenterica.	Consumption of young from diseased glands.

CLASSIFICATION OF DISEASES.

Classes.	Species.	Synonymes.
Dropsy.	Dropsy, general.	
	——— of chest.	
	——— of belly.	
Fever.		Typhus, continued fever, or nervous or putrid.
Head, diseases of	Apoplexy.	Rupture of vessel in, or overflow of blood to head.
	Palsy.	Paralysis.
	Water in the head.	Hydrocephalus.
	Inflammation of head.	Head or brain fever.
Hooping cough.		Chincough.
Heart, diseases of		Aneurism, ossification.
Inflammation.	General, or	Not specified.
	of chest.	
	— bowels.	
	— liver.	
	— throat.	
Measles.		
Nervous diseases.		Convulsions, fits, lock-jaw, epilepsy.
Scarlet Fever.		
Small-pox.		
Miscellaneous diseases.	Abscess lumbar.	Suppuration of the loins.
	Cancer.	Of breast, of lips, of bowels; schirrus.
	Carbuncle.	
	Diabetes.	
	Dyspepsia.	Stomach complaints.
	Erysipelas.	Rose.
	Flooding.	
	Jaundice.	
	Joints, diseased.	White swelling, diseased hip-joint.
	Rheumatism.	
	Rupture.	Hernia.
	Spine diseased.	
	Spitting of blood.	
	Stone in bladder.	

We here give the classification as it appears in the Appendix to the work before us, (page 128) to which is added the following note by the author, which we think sufficiently explains itself.

"The classification of diseases used in our mortality tables is far from being so complete as would be attainable were a legislative measure obtained for the improvement of the registers of deaths in Scotland. The above arrangement, though by no means so complete as could be wished, is possibly as much so as it can be made in the present state of our registers. This arrangement was first adopted for the mortality bill of 1835. In 1836 some slight alterations were introduced, at the suggestion of the late Dr Corkindale, who added the following note in reference to it; and the same plan has been uniformly followed in our Glasgow mortality bills since that date, and also in the tables contained in the 'Report on the Vital Statistics of Large Towns in Scotland,' published in the volume of the Transactions of the British Association for 1842.

"*Note by Dr Corkindale.*—'The species in the above form contain a list of diseases tolerably complete for the construction of mortality bills. There are, however, many affections omitted that are often the causes of death; for no list could be at all workable in any ordinary way, if it attempted to comprehend and specify every variety of disease. In the present circumstances of the preparation of these bills, this list is rendered still less complete by the necessity of using a classification called miscellaneous diseases, many of which, though having nothing in common, are grouped together, because they are not comprehended under the other classes.'"

There is next a very important table for each year, exhibiting the number of deaths monthly under five years of age, with the proportions these bear to the whole amount of deaths ascertained monthly under that age, and also to the whole population. The two succeeding tables shew the ascertained diseases which caused death in children under five years of age.

Tables 35, 36, 37 and 38 exhibit the number of burials and fatal cases of disease registered and occurring in the city and suburbs of Glasgow during 1843 and 1844, classified according to eighteen periods of life at which they took place; with calculations shewing the number of deaths and burials at each period, and the proportions which the whole burials and deaths from the several diseases bear to the total number of deaths and burials, and also to the estimated population.

Lastly, we have a most comprehensive and extremely interesting table, exhibiting the number of fatal cases of disease that occurred in the city and suburbs of Glasgow during the years 1838, 1839, 1840, 1841, 1842, 1843, and 1844, classified according to eighteen periods of life at which they took place; with calculations showing the total annual average number of deaths at each period, and the proportions which these, and the number of deaths from the several diseases, bear to the total average annual number of deaths, and also to the mean population for these years.

The indefatigable industry and application which must have been devoted to the construction of these tables, can be fully appreciated only by those engaged in similar pursuits; and though we have thus hastily sketched the outline of our author's compilations, we are fully alive to the careless manner in which many pass over tabular representations, without appreciating or recollecting the vast amount of labour, trouble, and calculation that has been bestowed upon them. Such persons forget the true object of a well constructed table, which is, to present at one view, by means of figures, results which cannot so well, or it may be, cannot at all, be expressed in words.

The author's remarks on the tables and their results form the second part of the treatise, and these may be advantageously considered under two heads; the first referring to the vitality, and the second to the mortality of the population, both subjects being illustrated by numerous additional tables.

It appears from the results of the second and fifth tables, that with regard to births and baptisms for the years 1843 and 1844, as engrossed in the registers, a decrease of 137, as compared with the year 1842, had taken place in 1843, and an increase of 217 over that year had occurred in 1844. These are the numbers engrossed; but the present mode of registering births in the city, the author observes, is utterly insufficient, and this prevails equally in other parts of Scotland, and for which there appears to be no remedy but the introduction of a government measure to insure accurate registration upon uniform and systematic principles. The increase of twin births in 1844 is remarkable; the number in 1842 being 36, in 1843, 42, and in 1844, 78.

Having shortly noticed the births and baptisms, Dr Watt proceeds to take up the subject of marriages registered for the years specified, and from his calculations it appears that an increase of 144 resident marriages had taken place in 1843 above the number in 1842, and of 377 in 1844 above 1843. He then goes on to show by a comparison of the marriages during the five preceding years, that their amount has fallen with the depression, and again risen with the prosperity of commerce in the city. He at the same time states that from an accumulation of facts obtained from various towns, it appears, that where there is the greatest amount of poverty and destitution, and where the mortality is greatest, the annual amount of marriages is highest; and that during years in which there was no scarcity of food, it being cheap, of good quality, and in abundant supply, but in which there was severe commercial distress, attended by a very limited demand for labour, there was a diminution in the proportionate amount of marriages both among the richer and poorer classes, showing that prudential motives, to a considerable extent, act as a check to the increase of marriages, and consequently of population. His results, he says, further prove, in accordance with Dr Alison's views, that this prudential check is much stronger in communities consisting of the better classes of society, than in those where there is the greatest amount of the wretchedly poor; and in illustration he gives a series of tables, showing the marriages for the five years from 1839 to 1843, in the three great divisions of the city and suburbs, namely, the city or parish of Glasgow, the parish of Barony, and the parish of Gorbals.

From these tables it appears, that on the average of five years, including those of the greatest commercial distress immediately preceding 1844, decidedly fewer marriages took place in the Barony,—the parish in which the largest proportion of the most wealthy reside,—than in the city. It is also observed that in 1842, the year of greatest commercial distress, though the proportion of marriages was considerably less in all the parishes of Glasgow, the decrease in the Barony was much greater than in the city.

From these statements, we conceive the author's opinion of this much disputed question to be, that it is during seasons of bad trade and commercial distress, accompanied by a very much diminished demand for labour, that the smallest amount of marriages will be found to be contracted; and, that it is not till the poverty and destitution which follow as a consequence of long continued depression, have set in, that they will be found to increase; the sufferers then becoming quite reckless and careless as to consequences, thinking only of the present, without any regard for the future, and considering themselves, as Dr Watt remarks, "entitled to marry as soon as they can earn 'men's wages,' without being first trained to husband their means for necessary purposes." He draws a marked line of distinction with regard to improvident marriages, between the wretchedly poor who are sunk to the lowest depths of poverty, and the more respectable portion of the mechanics and artisans of towns. In still further confirmation of the truth of this opinion, he goes on to notice the occupations of parties married, which, though not stated in the Gorbals register, are recorded in those of the City and Barony parish. A series of interesting tables is given, the results of which tend to show that the amount of marriages of bakers, boot and shoemakers, labourers, porters, messengers, masons, builders, tailors, weavers, joiners, and other artisans is directly influenced by the state of trade and commerce, being observed gradually to increase from 1842 to 1844; and that the greatest proportion of marriages takes place among the poorest classes of these in Glasgow, namely, the weavers and labourers, good and bad times seeming to have a more immediate effect on the amount of their marriages, than on those of any other class of workmen stated in the tables.

The marriages among clergymen, students of divinity, writers, attorneys, and law students, are, as might be expected, in nowise influenced by the state of trade and commerce; neither are those of physicians, surgeons, apothecaries, and medical students affected by their vicissitudes, although the following is stated respecting them.

> "It so happens, however, that in the city the greatest proportion of marriages among them was during 1843, the year in which there was the greatest amount of sickness and mortality: their marriages amounting to 5·47 per cent. In 1842, the most healthy of the three years, the marriages among them amounted to only 2·73 per cent. On examining the table for the Barony parish, it will be found that the smallest proportion of marriages among physicians, surgeons, &c., is for the year 1842, and during the other two years they are equal in amount."

The last point connected with the vitality of the population which Dr Watt takes up, is a very important one; namely, the increase of the population. His remarks are chiefly confined to the discussion of Mr Doubleday's theory, "that increase and decrease are in the inverse ratio with the quantity and quality of aliment;—that plenty produces sterility, and scarcity, fecundity." Into the merits of this question we do not propose at present to enter; but we cannot quit the subject of population, without devoting a single paragraph to the simple consideration of its very rapid increase in Glasgow.

If we look back to the statistics of population in this city for the last fifty or sixty years, we shall find that it has increased in the following proportions:—The average annual increase from 1791 to 1801 was 1719; from 1801 to 1811, 2669; from 1811 to 1821, 3658; from 1821 to 1831, 5538; from 1831 to 1841, 7970; and Dr Watt estimates the population in the city and suburbs in June 1843 at 301,000; and in 1844 at 311,600; the increase between these two years being not less than 10,600. Thus, we observe, the annual increase of population is double what it was five-and-twenty years ago, and it still advances in a rapidly increasing ratio. Dr Watt considers that the greatest increase of population takes place from immigration, as there is a greater excess of deaths over births in Glasgow than in other towns with which comparisons have been made. Of the truth of this we entertain little doubt; but we would ask, what becomes of the eight or ten thousand individuals annually added to the population? Where are they lodged? To what class of the community do they belong? And if to the poorer, is there employment for them? How do they exist or gain a living? We are inclined to think that it will be readily admitted that by far the greater number belong to the poorer classes of society. Much has been said, and much been written, about the crowding together of men, women, children, and families, in the lodging houses and cellars of the wynds and closes of Glasgow; and if any doubt of the fact were entertained, we have the proofs before us, as detailed above. It is true, Glasgow is rapidly extending; in every direction houses are being built; but for whose accommodation? Certainly not for the class of the community in most need of it, that class who cannot afford to pay a rent sufficient to insure them a comfortable lodging, and consequently are obliged to betake themselves to those over-crowded hovels, the receptacles of poverty, disease, and in many instances, of crime, in the densely populated parts of the city and suburbs. In this way year after year adds to the numbers that are nightly packed in these obnoxious abodes, and in many cases where extreme destitution and poverty were the causes which induced the unhappy sufferers first to take refuge there, there is too much reason to fear that the evil lessons and pernicious examples which they see before them, sink them to a state of moral degradation and recklessness, from which they never afterwards recover. We need not wonder then, such being the state of hundreds, that when an epidemic disease, typhus fever, for example, breaks out among such a living mass of "material" ready for its ravages, it should spread with fearful rapidity, carrying death and destruction along with it. It is anxiously to be hoped, that the enactments of the recent poor law for Scotland, and the power given to those who have the charge of enforcing it, will enable them soon to apply the proper remedies to these daily increasing evils.

We next proceed to the second division of the author's subject, viz. that of the mortality during the two years he has analysed, which, in a medical point of view, is perhaps the most important part of the treatise.

In estimating the comparative value of male and female life in Glasgow for the seven years preceding 1845, Dr Watt states that the latter is better than the former by 0.356 per cent. "The proportionate improvement of the male to the female life," he thinks, "on the average of these seven years, arises chiefly from the epidemic which prevailed during 1843, having, contrary to the usual course of epidemic fever in Glasgow, cut off a much larger proportion of females than of males, causing the total amount of female deaths for that year to exceed the male deaths by 3.72 per cent."

The following, taken from table forty-seventh, exhibits the amount of mortality to the population for the seven years preceding 1845.

			Per cent.
In 1838	the proportion of mortality to the population was	1 in 36.84	or 2.71
1839		1 35·08	or 2.85
1840		1 30.93	or 3.23
1841		1 31.75	or 3.14
1842		1 39.62	or 2.52
1843		1 31.82	or 3.14
1844		1 42.29	or 2.36

Dr Watt has devoted great attention to the mortality at the several ages for different diseases, and his observations tend to show that there are specific laws which appear to regulate its amount at different ages for each disease; and he thinks, "that were our data sufficiently complete to allow of the law of mortality at different ages being accurately ascertained for all diseases, then the total amount of deaths by each disease being given, they would enable us to calculate with precision the ages at which the total deaths in that locality had taken place."

The great mortality of children in Glasgow is a fact well worthy of attention. Dr Watt states the proportion of deaths to the living under one year as 18.90 per cent. in Glasgow, as 17.18 per cent. in Edinburgh, and as 15.69 per cent. in Perth; and this large amount of infantile death, he thinks, might be remedied were the children properly attended to, and supplied with plenty of nourishing food, comfortable clothing, air, and exercise. That this is the case is apparent from the fact that the greatest proportion of infant mortality occurs among the working classes. In proof of this he brings forward the evidence of the Rev. J. Clay, who states, that the infant mortality in Preston among the working classes is 55.5 per cent., while the gentry lose only 17.5 per cent., and that if better attention had been bestowed on the former "during the last six years, 3034 children would have reached five years of age, who, as it is, have been prematurely swept away by disease." The months most fatal to children under five years of age in Glasgow are, according to our author, August and September, and the month most favourable to the health of children, compared with the higher ages, appears to be April, yet the month in which the fewest deaths among children occur is June. August and September appear from the tables to be the most favourable months for adults from twenty to sixty, and June and October for the aged population. January is the most fatal month for adults of all ages.

In prosecution of this important subject, Dr Watt goes on to notice the physical laws which appear to govern the amount of deaths at different ages by the several diseases. He gives a series of well constructed tables, exhibiting the amount of deaths by measles, scarlatina, small-pox, hooping cough, croup, and fever, under different ages, in London during 1842, and in twenty-four town-districts in England during 1840, with their proportions per cent. to the whole deaths in the metropolis, and in these twenty-four districts respectively. It is extremely interesting to compare these per centages, and to remark how very closely they coincide, and we cannot help thinking that the prosecution of this subject will eventually lead to the most important results, and enable us in future to form a much more correct estimate of the public health than we have hitherto been able to arrive at.

Table fifty-eighth exhibits the number of fatal cases of the eruptive diseases, hooping cough, and fever, occurring in Glasgow and suburbs during 1843 and 1844, with the proportions which the number of these deaths at different ages bear to the whole deaths by each disease respectively, and is so instructive that we present it without abridgment.

Table Fifty-eighth.

Ages.	In Glasgow & suburbs, 1843.				In Glasgow & suburbs, 1844.			
	Deaths by various diseases under these ages.			Propor. to whole deaths by each dis. respectively.	Deaths by various diseases under these ages.			Propor. to whole deaths by each dis. respectively.
	M.	F	Tot.		M.	F.	Tot.	
CASES OF MEASLES.								
Under 2 years,	55	48	103	50·49	78	74	152	45·92
Under 5 years,	88	81	169	82·84	142	147	289	87·31
Under 20 years,	104	100	204	100·00	156	175	331	100·00
20 and upwards,	...	...	...	...	...	...	...	...
Total,	104	100	204	100·00	156	175	331	100·00
CASES OF SCARLET FEVER.								
Under 2 years,	30	26	56	22·40	58	59	117	22·50
Under 5 years,	75	74	149	59·60	157	170	327	62·88
Under 20 years,	129	114	243	97·20	252	261	513	98·65
20 and upwards,	1	6	7	2·80	2	5	7	1·34
Total,	130	120	250	100·00	254	266	520	100·00
CASES OF SMALL-POX.								
Under 2 years,	39	38	77	50·99	18	21	39	39·39
Under 5 years,	60	56	116	76·82	38	43	81	81·81
Under 20 years,	79	68	147	97·35	44	53	97	97·97
20 and upwards,	4	...	4	2·64	1	1	2	2·02
Total,	83	68	151	100·00	45	54	99	100·00
CASES OF HOOPING-COUGH.								
Under 2 years,	128	179	307	61·77	91	99	190	63·12
Under 5 years,	193	256	449	90·34	133	147	280	93·02
Under 20 years,	218	278	496	99·79	143	158	301	100·00
20 and upwards,	...	1	1	0·20	...	...	...	...
Total,	218	279	497	100·00	143	158	301	100·00
CASES OF FEVER.								
Under 2 years,	100	92	192	13·73	25	23	48	7·33
Under 5 years,	142	129	271	19·38	39	47	86	13·14
Under 20 years,	215	199	414	29·61	75	99	174	26·60
20 and upwards,	456	528	984	70·37	260	220	480	73·39
Total,	671	727	1398	100·00	335	319	654	100·00

The last points taken up by Dr Watt are the causes of excessive mortality. He here repeats his opinion expressed in his former publications, and confirmed by the experience and observations of Dr Alison and others, that destitution may be considered as the principal of these causes; and he, in particular, alludes to the year 1837 as a year of "great commercial distress, and of excessive mortality," when "about two-thirds of the increased number of deaths occurred exclusively among those who were so poor as to be buried at the public expense." The high mortality of 1843 he accounts for in the same way, showing that though there were between 30,000 and 40,000 affected with the epidemic of that year, and each of them for weeks at a time, still more offered themselves for employment than could obtain it. Another circumstance strongly corroborative of the influence of this cause is the fact, that, from evidence lately brought forward, " there is perhaps no town of a more healthy character than this city (Glasgow) for the wealthy, and those in comfortable circumstances."

Such, then, being the natural character of Glasgow for salubrity and healthfulness, we cannot resist the conclusion, that, when its mortality becomes excessive, and when we see this mortality uniformly in connection with depression in trade, and falling on the most destitute part of the population, it is to this depression, and the destitution which accompanies it, that we must look for an explanation, and consequently to their amelioration for a remedy.

We have already shown the rapidity with which the population is yearly increasing; and with this increase the crowding and packing of human beings into the dwellings and lodging-houses of the poor is annually as steadily advancing; and thus, whatever advantages as to climate, situation, and salubrity the city may possess, these are as effectually as possible done away with to this class of the community. Until, then, the state of the destitute poor be improved and placed on a better footing; until they have the advantages of drainage, ventilation, cleanliness, &c.; and until their habitations be made more like what the abodes of human beings should be, we may expect to have not only the same amount of misery and mortality which have at different seasons prevailed in Glasgow, but we must also be prepared to see it annually assume a much more aggravated and wretched character. We, therefore, cordially join with Dr Watt in urging upon the public authorities of Glasgow (and we also add, those of our own town) to press forward the sanatory improvements of their city; and to avail themselves as fully as they can of the powers with which the new poor-law has invested them.

As the subject of the health of towns is one to which the attention of government is specially to be called, it is with no little anxiety that we look forward to its deliberations; and we cherish the hope, that some decisive steps shall be taken to meet permanently the growing evils which we have just been considering. On this account, it is well for Glasgow that it possesses such a body of evidence on vital statistics, from which the true condition of its people can readily be arrived at, and the necessary conclusions drawn from them. It was with much pleasure and satisfaction that we observed that the Town Council of Edinburgh resolved to get tables of the mortality of the city and of Leith drawn up, the want of them having been long felt and complained of. We have regularly published these bills since their commencement, and we think it both a fortunate and a wise arrangement, that they are compiled on the same principle as the classified nosology of the Registrar-General for England, which, in a recent number of this Journal, we took an opportunity of examining. It would be highly desirable that those of Glasgow were published on a uniform plan with this nosology; and we take this opportunity of urging upon our author Dr Watt, the consideration of the possibility of effecting this object. Could this be accomplished we would, as far as possible, be assisting in making up for the want of a legislative enactment for Scotland, similar to that for England, which, it is to be hoped, will one day be extended to this country, where it would be hailed by many as a national benefit.

Without presuming to dictate, we cannot conclude without expressing an earnest wish, that the authorities of Edinburgh will urge their Statistical Registrar Dr Stark, to publish an *annual* compilation, showing the results of his labours for each year, in addition to the usual *monthly* returns; as, by this means, a more condensed view of the sanatory condition of the inhabitants will be given, and it will materially increase the value of the vital statistics for the purposes of comparison.

ART. II.—*Clinical Illustrations of the Diseases of India, as exhibited in the Medical History of a body of European Soldiers for a series of years from their arrival in that country.* By WILLIAM GEDDES, M. D., Member of the Royal Medical Society, Edinburgh, and late Surgeon of the Madras European Regiment. London, 1846. 8vo, pp. 492.

AMONG the European forces employed by the Honourable East India Company, there are at each of the three presidencies of Bengal, Madras, and Bombay, one European regiment of infantry, and a certain proportion of horse and foot artillery. These are recruited from time to time, as occasion, and the urgency of the service demands, by supplies from the British dominions.

The European regiment of infantry on the Madras establishment has existed as an embodied corps since the year 1748; and has now for a century formed a very important part of the military force of the colonial government. The corps had been actively engaged, and had lost many men by disease and in action during the Burmese war; and the survivors had returned from Ava, and disembarked at Masulipatam, on the sea coast of the Bay of Bengal, in July 1826. During the ensuing two years many debilitated and worn out men were discharged from the regiment, while to supply their place large accessions were made by recruits from England; so that on the 1st July 1828, when the present medical history commences, of 503 individuals in the regiment, only 155 had been in a warm climate beyond a period of three years.

At Masulipatam the regiment remained upwards of three years. It was then named the first Madras European regiment; but in the course of eighteen months the European infantry of each presidency being united into one regiment, this portion became the right wing of the Madras European regiment. In the course of the same year further additions of 191 recruits were made, which raised the numerical amount of the force to 694 men; and to this number the researches and returns of Dr Geddes in the present volume refer.

By a tabular statement given at page 4, it appears that the recruits had been derived in general from the labouring population of the three kingdoms; that nearly one-half (334) had been from the various classes of labourers in the open air; and of these 277 were registered simply as labourers, of whom 199 were of Irish origin. Of the remainder more than two-thirds (259) consisted of individuals who had been employed in labour or occupations within doors; one-half of whom had been attended with a constrained position of the body (126); as shoemakers, tailors, and weavers; the number of the last in the regiment amounting to 73. Forty-two persons were received from professions involving labour under an elevated temperature, as blacksmiths, and other smiths, bakers, and the like; thirty-seven from professions of a sedentary nature, or implying confinement to the house; fifteen from trades usually considered of a deleterious tendency; and the remaining seven individuals belonged to professions which could not properly be arranged under either of the above heads. Of the total 694, 363 were Irish, 287 English, 42 Scotch, and 2 foreigners.

Of the total number of 678 recruits 204 were received at 20 years of age; 95 at 21, 57 at 23, 53 at 25, 51 at 22, 45 at 26, 40 at 19; the others having entered at ages too irregular to be rendered the subject of exact notice. The ages of 20 and 21, therefore, appear to be the predominant periods at which recruits are received for the service. But as this must depend not only on the numbers offering themselves, but on those passed and refused, it merely shows that after a certain age men are not received, or are sparingly and cautiously admitted as soldiers.

The rate of mortality is also greatest at the ages of 20 and 21. At the former age it appears that 31 died in hospital, at the latter 24; and if the two ages be taken together, the mortality was

55 among 299 men, which amounts to 183 per 1000, or between one-fifth and one-tenth. Among the soldiers at 25, however, the mortality is also considerable; being 11 among 53, or fully one-fifth.

This body of men the author joined as surgeon on the 13th of May 1829, and remained in medical charge of it to the same day of 1833, forming a period of four years, to which alone his personal knowledge of the corps applies. The medical history of this regiment, nevertheless, has been extended from the official returns to include a space of five years, beginning, as already stated, from the 1st of July 1828. Two periods are, therefore, referred to in the work. One of these extends to five years, as already stated; the other embracing only four years, and corresponding with the personal observation of the author.

The regiment appears to have remained at Masulipatam from the month of July 1826 to the 15th of December 1829, on which day it marched from this place on route to Kamptee, and arrived there on the 5th of March 1830. There it remained till the 1st July 1833.

The coast of the Bay of Bengal, upon which Masulipatam is situate, has a general or north-easterly course along the Northern Circars. But immediately north of the mouth of the river Kisthna, the shore takes a bend towards the west, forming an extensive bay, where the water gradually shallows as it approaches the land; an effect which is ascribed to alluvial deposit from the river. The fort of Masulipatam, which lies in latitude 16° 9′ 8″, and east longitude 81° 11′ 38″, had been originally constructed by the Dutch. It is situate on the north bank of a salt water creek, which issues from the bay about 2000 yards south-east of the fort. This inlet, dilating shortly after its commencement to the breadth of 400 yards, extends into the interior of the country, through the level alluvial deposit already mentioned. It gives off a communication to the ditch surrounding the fort; and varying in extent according to the state of the tide, its waters are admitted into an extensive flat to the northward of the place. This introduction of sea water gives a saline impregnation and interrupts vegetation, except that of maritime plants, as the *sali corniæ.* This flat saline swamp surrounds the fort to a distance varying from 2000 to 3000 yards on all sides. In the dry season it forms an arid level, with a saline crust on its surface, and presenting in great perfection the phenomena of the mirage, when the vapours of the soil are raised during the heat of the day. In some points near the fort are pools of salt water, which vary according to the state of the tide. In the wet season, which in certain years continues from June to December, the whole of this flat is covered almost entirely by water, derived chiefly from the rains which fall in the neighbourhood. The fort forms an irregular parallelogram of 1000 yards running north and south by 750 east and west, and is surrounded by a broad ditch containing salt water. The interior is little raised above the surface of the surrounding country. It is represented, nevertheless, to be provided with broad streets and open spaces sufficient to insure ventilation. Fresh water is brought for culinary purposes from a distance of about 2500 yards, where the soil beyond the swamp consisted chiefly of sand.

From Masulipatam to Kamptee the course of march is northward and westward to the distance of 520 miles; first about 40 miles from the sea coast through a black alluvial deposit, named cotton ground, and which is supposed to consist of disintegrated trap rocks with animal and vegetable remains; then for 200 miles through a primitive formation, with a soil consisting of the debris of granitic and syenitic rocks,—the road rising gradually to the height of nearly 2000 feet above the level of the sea; and, thirdly, descending through a district of trap rocks for about 160 miles; and, lastly, for about 100 miles through a district of low hills, varying from 600 to 900 feet above the level of the sea. At a short distance from Kamptee the country becomes again primary; and granite, sandstone ? and limestone rocks, the latter we infer primitive, are seen on the bed of the river Kanar, on which the cantonment is situate.

Kamptee is equidistant from the east and west coasts of the Indian peninsula, being about 400 miles distant from either. Its latitude is 21° 16′ north, longitude 79° 46′ east; and it is placed about 900 feet above the level of the sea. It stands at the distance of 9 miles to the north of the city of Nagpoor; and such is the disposition of the surface, that on approaching from Nagpoor, Kamptee is not seen, until the spectator reaches the verge of a rising-ground running parallel to the cantonment, at a distance of about half a mile. The cantonment is situate on the right bank of the river Kanar, a considerable stream, which, joining its waters to those of the Godavery, falls into the bay of Bengal. At Kamptee, where it forms a considerable bend, it runs over a sandy bed with much rapidity; yet, excepting in the floods of the rainy season, it is generally fordable and free from any muddy intermixture. The right bank rises precipitously to the height of from 50 to 60 feet above the low water level; and on this bank, the soil of which is clay over strata of sand and rolled stones, the houses of the cantonment are erected.

The site of the barracks in which the soldiers were quartered, both at Masulipatam and Kamptee, partook of the physical characters of the district. At Masulipatam they consist of extensive one-storied buildings on a level with the area of the fort, and were exposed to a high degree of heat during the day and dry season, while in the rainy season they were often damp or close and sultry. They were, nevertheless, open to the sea breeze. At Kamptee the barracks form a conspicuous object, consisting of several one-storied tiled buildings, raised a few feet from the ground.

The food of these European troops is substantial and abundant, consisting of a much larger proportion of animal food than is used by the same classes in their native country. Capsicum and similar stimulating condiments are in general and abundant use; and with them rice is also largely used. The drinks consist of sour coagulated milk, butter-milk, tamarind-water, lime-juice, and a certain proportion of arrack,—the amount of the latter consumed by each soldier being equal to two measures or drams. Brandy, gin, wine, and beer are also consumed to a considerable extent.

The author presents in a series of tables the admissions and health and disease movements of the corps during the space of five years; and from these the following deductions are given.

" It appears that an average strength of 533 European soldiers afforded an annual average of 1332 admissions to hospital for a space of five years; or a monthly average of 111 cases of disease. This exhibits a degree of sickness equal to 2½ admissions to each soldier within the year; and their stay in hospital appearing, on an average of the whole to have been 12½ days, the mean loss to the service from the confinement there, throughout the above period, was at the annual rate of about 31 days, or, in other words, one month in twelve. Again, on an examination of the observations from which these averages are taken, it appears, with regard to the number of times each individual was in hospital during the five years alluded to, that these varied from once to 45 times. Of those men who were present at head-quarters during the whole of this period, four persons only had not been in hospital within this time; and of these, also, two had been unwell, but from their situation as staff-sergeants, they had not been placed on the sick-list. Seventy-two soldiers had been in hospital from one to five times within the five years; 137 from six to ten times; 107 from 11 to 15 times; 79 from 16 to 20 times; 31 from 21 to 25 times; and the remaining 12 a greater number of times. Of these, two were 44 and 45 times in hospital, and they (as well as, in general, the remainder of the 12) owed their frequent admissions upon the sick-list to disorders the effect of their own imprudence, and chiefly of a trifling description. With respect to the number of days they remained in hospital, the same variety existed. Of those who had been at any time in hospital, 32 had been there from one to twenty days, within the five years; 39 from 21 to 40 days; 36 from 41 to 60; 43 from 61 to 80; 52 from 81 to 100; 78 from 100 to 150; 65 from 150 to 200; 63 from 200 to 300; 19 from 300 to 400 days; and the remaining 11 for longer periods. In only two of the latter do they correspond with any of the 12 who had the greatest number of admissions to hospital. The remainder owe their lengthened stay there, chiefly to extensive ulcers or rheumatic affections.

"Classing the whole body of men, as originally existing, into two nearly equal divisions—one of which shall consist of the labourers and weavers, amounting to 346, and the other of those who had been more especially brought up to some profession, amounting to 348—the average number of times in hospital to each individual of those who remained with the corps on the 1st of July 1833, is found to be somewhat greater among the former than in the latter. It is also greater in those who arrived in India after the 1st of July 1828, and whose diseases are accordingly noted from the period of their arrival, than in those who had joined the regiment before that time. Thus, of 217 individuals of the first class remaining on the 1st of July 1833, the average number of admissions in the preceding five years had been slightly beyond 12; while of 225 of the second, the average had been in the same proportion, above 11 times. Again, in those of the first class who had been in the corps before the 1st of July 1828, the average is under 12 times; while that of the individuals who joined afterwards amounted to 15 times. In the second class, the average of the earlier arrivals was not 11 times; while that of the later was 13 times. It would appear, also —as far as can be ascertained by an observation of an excess in the average number of admissions—that, generally speaking, in all classes, individuals arriving at the ages of 21 and 26 were more frequently in hospital than those of other ages.

"The most important diseases exhibited in the tables which have been given above, are Fever, Hepatic disease, Abdominal and Cephalic Inflammation Rheumatism, Dysentery, and Diarrhœa, Cholera, and Indigestion, together with Syphilis, local and some other diseases. It may be worth while to examine the proportion which the admissions to hospital with each of the above forms of morbid affection bear to each other throughout the five years; and also the degree in which the absence from duty of the soldier is to be referred to each class of diseases, It appears that of 6,590 cases admitted into hospital from 694 individuals—of which number this body of men was originally composed,—nearly one in every four cases was affected with Fever; and that a like proportion prevailed of local complaints; therefore nearly one-half of the whole number was admitted with one or the other of these forms of disease. The Dysenteric disorders range next in order of frequency, being one in every eleven; then follow Rheumatism, Indigestion, and the class of other diseases, being each one in 12. To these succeed Syphilis, one in 14; Cephalic Inflammation one in 16; Diarrhœa, one in 23; Hepatic disease, one in 24; Abdominal Inflammation, one in 61, and lastly,—though certainly not the least important of the whole—the Epidemic Cholera, cases of which occurred as one to 74 of those of other disorders. Of all these, the class of local diseases, compared to any of the others, was the predominating cause of absence from duty. The average duration in hospital of each case of these diseases was 15 days; and the total number of days' residence there, occasioned by this source, amounted to rather more than a fourth of that induced by the prevalence of all other complaints. Again, the average stay in hospital of each of the cases of Fever being only six days, the absence from duty occasioned by this cause did not extend beyond one-seventh of that produced by all other diseases. Rheumatism and Syphilis nearly equalled Fever in this respect, being respectively one-eighth; the average duration in hospital of each case of the former, being 17, and of the latter, 21 days. That of the Dysenteric cases was 15 days; and the absence from duty occasioned by this disease equalled one-ninth of the whole. Then follows the class of other diseases, the average stay in hospital occasioned by each of which amounted to 13 days; and the total residence there formed one-eleventh of all other complaints. To these succeed Hepatic diseases, with an average stay in hospital of 21 days, forming one-fourteenth of other disorders; Indigestion having an average of six days in hospital, and a residence there amounting to one-twenty-fifth of all the others; Diarrhœa, which amounted to one-thirty-fifth, with an average stay of six days in hospital; and Abdominal Inflammation, which formed one-fifty-second part, with an average residence in hospital of 14 days. The remaining classes of Cephalic Inflammation and Cholera, comprehend one-eight-second and one-ninety-ninth portion of the absence from duty occasioned by all diseases; the average stay in hospital with instances of the former affection, being a period of eight, and of the latter, of nine days.

" The existence of so many young soldiers in this body of men—with the opportunities afforded by the hospital records, of ascertaining, accurately, the state of health of each individual from the period of his arrival in India—gives sufficient grounds to prove, that, in usual circumstances, there is in that country no disease of the nature of what has been named a seasoning one; which is usually understood to affect Europeans at some period shortly after their arrival in the warm climates of the west. It is true, indeed, that for the first few months of his residence in India, the young soldier is in general more frequently on the sick-list than at an after period of his sojourn there. The diseases which are the occasion of this circumstance are, however, seldom of the nature of that which has received the name of a seasoning one; and may often be referred to causes connected with the drill necessary for the recruit, or with the change of habit and food consequent to his landing, after a long voyage, as a soldier, in a country so widely different from his own as that of India. In 518 of the above new arrivals, 73 persons had been admitted into hospital within 40 days from their landing in India; 163 in from 40 to 100 days; 86 in from 100 to 160 days; 81 thence to 250 days; 40 from that period to 12 months; and the remaining 75 were not in hospital until after a greater length of time. The first diseases of the 236 individuals who were admitted within 100 days after their arrival in India, were as follow:—Fever, 7; Dysentery, 25; Hepatic Disease, 5; 2 Abdominal, and 1 Cephalic, Inflammation; Diarrhœa, 25; Rheumatism, 12; Dyspepsia, 13; Syphilis, 21; Local Complaints, 70;

and a variety of minor complaints, such as Catarrh, Constipation, &c., amounting to 55. The first diseases of 167 who were in hospital for the first time, in from 100 to 250 days after their arrival, were, Fever, 15; Dysentery, 8; Hepatic Diseases, 4; Abdominal Inflammation, 2; Cephalic Inflammation, 1; Diarrhœa, 10; Rheumatism, 7; Dyspepsia, 22; Cholera, 2; Syphilis, 15; Local Diseases, 56; and other complaints, 25. Of the remaining 115, who were not in hospital until after a period of 250 days from their arrival—some of them not being on the sick-list until the season in which fever prevailed very generally at Kamptee—the first diseases were:—Fever, 32; Dysentery, 10; Hepatic Diseases, 4; Abdominal Inflammation, 2; Diarrhœa, 4; Rheumatism, 6; Dyspepsia, 8; Cholera, 2; Syphilis, 7; Local Diseases, 32; and others, 8. Hence it appears that there is a greater proportion of local diseases, Diarrhœa, and the class of peculiar complaints, among the earlier causes of admission to hospital, than in future attacks of illness; that Dysentery, Indigestion, or Syphilis, are nearly equally numerous, either as first or after diseases; and that Fever, Hepatic Disease, and Rheumatism, are more frequently met with after the patient has been once or oftener in hospital than among his earlier diseases. It will, at the same time, readily suggest itself, that the season of the recruit's arrival in India, as well as the station to which he proceeds, must affect the proportion in which certain diseases are likely to attack him; and determine the period, from arrival, at which he has the greatest chance of being on the sick-list. In these respects, he must, in general, be considered as under the same influence as his more matured comrades.

"It further remains to be added, that of those men who had been on the sick-list more than once during the five years, the average period between each admission to hospital amounted to 138 days, or about four and a-half months. It may also be remarked, that the period of least sickness appears to have been always the cool season; that of the greatest number of admissions to hospital was, on the sea-coast, the hot season; and, during the rains and for a short period afterwards, at the inland station; while the greatest mortality—unless where caused by Cholera—was, for the most part, met with in the rainy season.

"The number of deaths included in the above tables of disease, as having occurred in hospital from 1st July 1828, to 1st July 1833, amounts to 133; being one in 57 of the sick under treatment, or somewhat less than two per cent. To these, however, ought properly to be added seven individuals, who either died a natural death before being brought to hospital, or whose diseases—contracted while at head-quarters—proved fatal, when the persons affected had been sent to a distance for the benefit of their health. The whole, being 140, forms the mortality from natural causes, in the course of five years, in a body of Europeans in India, averaging 533 individuals; this approximates to 26 per cent. in this period, or an average of five per cent. of the numerical strength in each year. Deaths from Cholera, however, being removed, the rate of mortality is reduced to 20 per cent.: or an annual average of four per cent. On a reference to the table given at page 4, it will be observed that of the respective nations of which this body was composed, 75 Irish, 56 English, and 9 Scotchmen died within the five years. This mortality in each nation is at an equal rate in the two former of 25 per cent.; but in the latter it amounts to about 33 per cent.: a circumstance chiefly to be referred to a larger proportion of deaths in these from Cholera, than among the English or Irish of the corps. It will also be seen from the same table that, in those who had been labourers in the open air, 61 deaths occurred,—44 among the Irishmen, and 17 among the English. Of those who had followed trades within doors, 69 persons died: viz. 30 Irishmen, 31 English, and 8 Scotch; and of these, 30 had been of professions attended with a constrained position of the body; 11 had been occupied in labour accompanied with exposure to heat; and 28 had been otherwise employed. Two Englishmen had been of trades usually considered of a deleterious description; and eight individuals, six of whom were English, one Irish, and one Scotch, had been occupied in simple sedentary employments. In all these classes, the mortality nearly corresponds; ranging from 20 to 24 per cent.; with the exception of those who had belonged to professions attended with a constrained position, or with exposure to heat, in which the fatal cases amounted to 32 per cent. in the course of five years. The nature of the diseases by which these deaths were produced will, in some measure, have been seen from the tables given of the admissions to hospital in each year; and will be more particularly referred to in another part of this work. Here it may be sufficient to state generally, that 31 persons died from Cholera; 27 from Abscess in the Liver; 25 from Dysenteric disorders; 19 from Affections of the Head; and the remainder from a variety of other diseases."—Pp. 74—82.

Fever occupies an important and conspicuous place in the sickliness of this regiment; and especially remittent and intermittent fever, or what the author terms *Paroxysmal* Fever, in contradistinction to fevers of continued type.

"On reverting to the table given at page 87, it will be seen that the number of Paroxysmal Fevers reported to have occurred within the five years to which these observations refer, amounted to 1,549. The distribution of these fevers over the period alluded to, will also be seen from the same document; by which it appears that the prevalence of this disease during the stay of the regiment on the sea-coast was of a trifling nature, compared to that of the period commencing with the first rainy season at Kamptee. The following facts are also sufficiently evident relative to the prevalence of the disease at this station: 1st, that fever occurred in greater numbers during the months of August, September, and October of each year than at other seasons; 2d, that it gradually diminished in frequency through the cold months, being in least numbers in that of February, and increased in an irregular manner through the hot season; and 3dly, that the year commencing from July, 1831, presented the greatest number of cases of the disease; while the following, or that from July, 1832, to the same month of 1833, contained the least number of admissions with Paroxysmal Fever.

"During the four years, from May, 1829, to May, 1833, of the author's charge in the Madras European Regiment, the number of cases of Remittent and Intermittent Fever reported to have occurred, amounted to 1,406. Some of these cases were attended by other medical officers doing duty at the time with the corps. Others, on a revision of their symptoms, accompanied by a consideration of the patient's health before or after the attack in question, have been transferred to a different disease; and a few have, on the other hand, been added by the operation of the same process. The number of cases, accordingly, arranged under these forms of fever, and the details of which are in possession of the author, having been under his own immediate charge, amounts to 1,210. These being generally diffused over the period alluded to, in proportion to the prevalence of the disease at the time, it is considered that a sufficiently accurate view of the nature and extent of Paroxysmal Fever, as it occurred in this body of men within the period mentioned, will be obtained by the author's observations being confined to such facts as came within his own cognisance, and are still within his reach. It is proposed, at the same time, to endeavour further to elucidate the nature and history of these diseases by a reference to the experience and inquiries of the author on other occasions. From the table* given opposite, compiled from the details just alluded to, the distribution of these cases over the period of observation will be seen to exhibit the same features of increase and decrease in the prevalence of fever that have already been made evident in the table at page 87. It is therefore unnecessary to revert to those circumstances here, further than to say—in relation to the meteorological details of different years given in a preceding part of this volume—that the rainy season being invariably most productive of Paroxysmal Fever, the number of cases of this disease was in proportion to the quantity of rain which fell: aided, perhaps, by the circumstances of an intensely hot season previously, or the occurrence of an interval of dry and comparatively warm weather after a heavy fall of rain at an early period of the monsoon. It also appears that the cold season—particularly that part of it at the greatest distance from the rains—was the most free from fever; and that its prevalence during the hot months was in proportion to the variation of temperature, within a certain range, in the 24 hours. This, again, depended partly on the extent and duration of the rains of the previous monsoon; and partly on the occurrence of rains during the hot season.

"It will be observed, from the number of cases stated to be in the writer's possession, that they must, necessarily, be composed partly of original attacks and partly of relapses. By the former is meant, the first occurrence of Paroxysmal Fever to a patient with-

* TABLE showing the Prevalence of FEVER and their TYPES in 1210 Cases, from 13th May 1829, to 13th May 1833.

	1829-30.								1830-1.								1831-2.								1832-3.								Total.							
	Quotidian remittent.	Quotidian intermittent.	Tertian remittent.	Tertian intermittent.	Quartan.	Double tertian remittent.	Double tertian intermittent.	Total.	Quotidian remittent.	Quotidian intermittent.	Tertian remittent.	Tertian intermittent.	Quartan.	Double tertian remittent.	Double tertian intermittent.	Total.	Quotidian remittent.	Quotidian intermittent.	Tertian remittent.	Tertian intermittent.	Quartan.	Double tertian remittent.	Double tertian intermittent.	Total.	Quotidian remittent.	Quotidian intermittent.	Tertian remittent.	Tertian intermittent.	Quartan.	Double tertian remittent.	Double tertian intermittent.	Total.	Quotidian remittent.	Quotidian intermittent.	Tertian remittent.	Tertian intermittent.	Quartan.	Double tertian remittent.	Double tertian intermittent.	Total.
May . . .	...	3	1	...	...	1	1	6	...	2	1	5	...	3	1	12	3	2	4	7	...	2	2	20	4	5	1	17	...	1	3	31	7	12	7	29	...	7	7	69
June . . .	1	...	...	2	...	...	...	3	1	...	2	6	...	1	...	10	4	8	4	11	1	2	1	31	9	5	5	16	...	1	4	40	15	13	11	35	1	4	5	84
July . . .	...	...	...	...	...	1	...	1	7	3	5	5	...	3	2	25	5	5	4	6	...	3	4	27	6	9	7	13	1	2	2	40	18	17	16	24	1	9	8	93
August . .	...	...	...	...	...	2	1	3	12	6	6	15	...	3	8	50	10	17	14	23	...	8	8	80	7	6	1	15	...	3	1	33	29	29	21	53	...	16	18	166
September . .	1	1	3	1	...	...	...	6	22	21	18	19	...	23	12	115	19	17	19	20	...	9	13	97	8	5	4	9	...	3	...	29	50	44	44	49	..	35	25	247
October . .	...	2	2	...	...	...	...	4	16	11	8	12	1	14	14	76	20	16	20	21	...	12	6	95	3	8	3	14	...	2	...	30	39	37	33	47	1	28	20	205
November . .	...	...	...	...	...	...	...	...	5	3	3	4	1	5	5	26	13	12	11	11	...	8	8	63	6	2	3	6	1	2	1	21	24	17	17	21	2	15	14	110
December . .	1	...	1	1	...	...	...	3	3	...	5	4	1	1	2	16	2	7	4	11	...	4	2	30	...	2	2	7	...	1	...	12	6	9	12	23	1	6	4	61
January . .	...	1	2	2	...	...	...	5	...	2	3	4	1	1	1	12	3	2	4	8	1	2	2	22	1	4	...	4	...	...	2	11	4	9	9	18	2	3	5	50
February . .	...	1	...	...	...	...	...	1	...	4	3	2	...	2	1	12	5	1	...	1	1	1	...	9	1	2	1	2	1	...	...	7	6	8	4	5	2	3	1	29
March . . .	3	2	1	2	...	3	1	12	1	2	3	4	1	4	4	19	1	...	1	3	...	1	1	7	1	2	...	3	...	3	...	9	6	6	5	12	1	11	6	47
April . . .	1	2	...	3	...	3	2	11	1	4	3	5	...	1	...	14	...	2	3	5	2	1	1	14	4	2	...	1	...	1	2	10	6	10	6	14	2	6	5	49
	7	12	10	11	...	10	5	55	68	58	60	85	5	61	50	387	85	89	88	127	5	53	45	495	50	52	27	107	3	19	15	273	210	211	185	330	13	143	118	1210

in the author's observation: and—with the exception of a few old soldiers, who may have been affected with this disease at some period of their service—prior to the arrival of the corps at Masulipatam, it is believed that few (if any) of the men had been the subjects of Intermittent or Remittent Fever previous to May, 1829. From this period until May, 1833—as will be observed by a reference to the table given on the opposite page—405 individuals had suffered one or more attacks of one or other of these forms of the disease. Of these, 22 persons had been attacked before the arrival of the regiment at Kamptee, in March, 1830. During the following twelve months, until the comparative cessation of the disease in February, 1831, 213 additional individuals had been seized; forming 225 who had been at this period the subjects of fever. In the following year 138 original attacks occurred; and 32 in that ending in March, 1833: at which period, 118 individuals still remained who had escaped the disease altogether. The same table exhibits the months in which these circumstances took place, as well as the proportion which the relapses bear to the original cases. It appears from this document that, during the earlier months of greatest prevalence of the disease, and of the residence at Kamptee, the admissions to hospital were (as might have been expected), proportionately more frequently composed of first, than of after attacks; and that towards the end of each unhealthy season—increasing in proportion as the men had been affected with fever—the predominance became, in a greater degree, on the side of the relapsed attacks. The latter amounted, within the four years, to 850; being somewhat less than twice the number of the original cases. A distinction might have been made in this class, between those more properly entitled to the designation of relapsed cases—as occurring within the same season as the original attack—and those taking place in the following season, or after a longer interval. It is conceived, indeed, that such a period might elapse between the first and second, or any other attacks, that the patient should be considered equally as free from any tendency to a return of the disease, as if he had never suffered from it; and that his next seizure, therefore, should properly be considered as an original one. Adverting, however, to the usual similarity of the diseases in original and relapsed cases, it has been considered best to follow the plan which has been adopted, of placing all the seizures subsequent to the first one, among the relapses; and the statement, to be immediatly given, of the period of time between each attack, will afford an idea of how far the tendency to fever may have been lost within the interval preceding the following return. Of the 405 individuals who had been at any period of the four years affected with fever, 119 were only once seized with the disease within this space of time; in 111, two attacks took place; in 59, three attacks; in 33 four attacks; in 25, five attacks; in 16, six attacks; in 17, seven; in 14, eight; in 6, nine; in 2, ten; in a like number, 11; and one soldier had 12 attacks. The interval between each attack will be ob-

* TABLE showing the relation of RELAPSES to ORIGINAL ATTACKS, and the periods of their Occurrence, &c.

	1829-30.					1830-1.							
	1st attack.	2d attack.	3d attack.	4th attack.	Total.	1st attack.	2d attack.	3d attack.	4th attack.	5th attack.	6th attack.	7th attack.	Total.
May . .	6	...	...	...	6	9	...	...	3	...	...	...	12
June . .	2	1	...	...	3	5	3	1	...	1	...	...	10
July . .	...	1	...	...	1	22	1	1	...	...	1	...	25
August . .	...	2	1	...	3	36	11	1	2	...	...	...	50
September	5	1	...	...	6	78	24	10	...	2	...	1	115
October .	3	1	...	...	4	32	28	9	5	2	...	...	76
November	...	...	...	...	...	7	5	11	2	1	...	...	26
December .	3	...	...	...	3	5	4	3	2	1	1	...	16
January .	3	1	1	...	5	2	4	2	1	1	1	1	12
February .	...	1	...	...	1	3	4	2	4	...	...	...	12
March .	8	3	...	1	12	5	3	4	6	3	...	...	19
April . .	7	1	3	...	11	3	1	3	2	1	1	...	14
	37	12	5	1	55	207	88	47	37	12	4	2	387

	1831-2.										
	1st attack.	2d attack.	3d attack.	4th attack.	5th attack.	6th attack.	7th attack.	8th attack.	9th attack.	10th attack.	Total.
May . .	2	8	5	1	2	1	1	...	...	...	20
June . .	8	10	6	5	1	...	...	1	...	...	31
July . .	8	8	4	4	2	1	...	...	...	...	27
August . .	34	21	7	6	6	2	2	1	1	...	80
September	31	27	18	9	7	3	...	2	...	...	97
October .	31	23	11	10	11	7	2	...	...	...	95
November	10	13	12	5	6	7	8	1	1	...	63
December .	3	5	8	3	1	3	4	3	...	...	30
January .	2	4	3	5	1	1	2	2	2	...	22
February .	...	1	2	4	...	...	...	...	1	1	9
March .	1	2	1	...	2	...	...	1	...	...	7
April . .	1	6	2	1	2	...	1	...	1	...	14
	131	128	79	53	[illegible]	25	20	11	6	1	496

	1832-3.												
	1st attack.	2d attack.	3d attack.	4th attack.	5th attack.	6th attack.	7th attack.	8th attack.	9th attack.	10th attack.	11th attack.	12th attack.	Total.
May . .	1	8	6	5	4	1	2	2	1	1	...	...	31
June . .	3	10	10	3	6	6	...	1	...	1	...	...	40
July . .	8	8	7	4	5	4	3	...	1	...	...	...	40
August . .	7	10	4	5	2	2	2	1	...	...	...	...	33
September	5	1	7	3	4	1	3	3	1	1	...	...	29
October .	3	6	5	3	2	4	1	1	1	1	2	1	30
November	1	5	2	3	2	2	2	3	...	...	1	...	21
December .	1	1	1	1	3	3	1	...	1	...	...	...	12
January .	...	5	1	2	...	1	1	1	...	...	...	...	11
February .	1	3	...	1	...	2	...	...	...	...	...	...	7
March .	...	1	1	3	1	2	1	...	...	...	...	...	9
April . .	...	...	1	1	1	1	4	2	...	...	...	...	10
	30	58	45	34	30	29	20	14	5	4	3	1	273

	Total												
	1st attack.	2d attack.	3d attack.	4th attack.	5th attack.	6th attack.	7th attack.	8th attack.	9th attack.	10th attack.	11th attack.	12th attack.	Total.
May . .	18	16	11	9	6	2	3	2	1	1	...	...	69
June . .	18	24	17	8	8	6	...	2	...	1	...	...	84
July . .	38	18	12	8	7	6	3	...	1	...	...	...	98
August . .	77	44	13	13	8	4	4	2	1	...	...	...	166
September	119	53	35	12	13	4	4	5	1	1	...	...	247
October .	69	58	25	18	15	11	3	1	1	1	2	1	205
November	18	23	25	10	9	9	10	4	1	...	1	...	110
December .	12	10	12	6	5	7	5	3	1	...	...	...	61
January .	7	14	7	8	2	8	4	3	2	...	...	...	50
February .	4	8	4	9	...	2	...	...	1	1	...	...	29
March .	14	7	6	10	6	2	1	1	...	...	...	...	47
April . .	11	11	9	4	4	2	5	2	1	...	...	...	49
	405	286	176	115	83	58	42	25	11	5	3	1	1210

served from the table* given below: it also shows the month in which the relapse took place; to which the intervening portion of time from the previous seizure has reference. It will be remarked, from a consideration of this table, that, as certain months of the year exhibit a greater tendency to the disease than others, the interval between each relapse varies, in some degree, from the circumstance of the patient's suffering during the unhealthy season; or from the period of this season at which the previous attack had occurred. Thus, of 495 cases of relapse which took place in the last five months of the year, 293 occurred within two months of the former attack; while of 310 relapses which were admitted into hospital during the remainder of the year, there were only 84 instances in which a longer period had not elapsed from the previous attack. Relapses, therefore, like the original attack, are most liable to occur in the rainy season; and it appears that at this time the most usual period between each return of the disease is from 15 to 30 days; while a large portion of cases also take place either in a less interval, or after a longer period—on to two months. One-half, however, of the whole relapses occurred at longer intervals than two months. Of these, also about an equal number took place in the unhealthy, and in the more healthy portion of the year; but it will be observed, that in the former a much larger proportion took place after a long period of immunity than in the healthy months; giving rise in many instances to the idea of a new disease having been excited. Thus, of 207 relapses occurring in the latter season, 149 took place within nine months of the previous attack, and 58 at a longer interval; while of 202 admissions in the unhealthy portion of the year, 99 took place in the shorter period, and 103 at an interval of more than nine months: 59 of these having been upwards of twelve months free from the disease. The number of relapses occurring within any one year, refers also, in some degree, to the period of the season at which the disease begins to attack the patient. Thus, by the table on the opposite page,

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* Table showing the Number of Days between each Attack, and the Month in which the last of the two occurred.

	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Total.
From 1 to 15 days	5	1	3	4	5	1	5	9	18	34	23	4	112
„ 15 to 30 do.	6	4	5	4	5	16	7	15	36	39	19	18	174
„ 1 to 2 months	8	5	2	2	4	4	7	12	15	21	17	13	110
„ 2 to 3 do.	7	3	3	1	3	2	3	7	7	3	16	5	60
„ 3 to 4 do.	8	...	4	2	2	2	3	2	8	7	...	1	39
„ 4 to 6 do.	3	6	5	8	6	4	4	4	4	7	1	3	55
„ 6 to 9 do.	1	2	6	13	20	20	8	5	8	3	4	4	94
„ 9 months to 1 year	1	1	...	2	2	12	12	19	16	8	1	...	74
Above 1 year	4	2	5	2	4	5	6	16	16	15	9	3	87
	43	24	33	38	51	66	55	89	128	137	90	51	805

* TABLE showing the Number of FIRST ATTACKS in each Year, and of the RELAPSES of each within the same Year.

	1829–30.				1830–1.								1831–2.									1832–3.					Total.								
	1 attack.	2 attacks.	3 attacks.	Total.	1 attack.	2 attacks.	3 attacks.	4 attacks.	5 attacks.	6 attacks.	7 attacks.	Total.	1 attack.	2 attacks.	3 attacks.	4 attacks.	5 attacks.	6 attacks.	7 attacks.	8 attacks.	Total.	1 attack.	2 attacks.	3 attacks.	4 attacks.	Total.	1 attack.	2 attacks.	3 attacks.	4 attacks.	5 attacks.	6 attacks.	7 attacks.	8 attacks.	Total.
January	...	...	...	...	2	...	1	...	...	...	...	3	1	...	...	1	...	...	...	...	2	1	...	1	...	2	4	...	2	1	...	...	...	...	7
February	...	...	...	...	...	...	...	...	...	...	...	...	2	1	...	...	...	...	...	...	3	1	...	...	...	1	3	1	...	..	...	...	...	..	4
March	...	...	...	..	2	1	...	2	2	...	1	8	4	1	...	...	...	...	...	...	5	..	1	...	...	1	6	3	...	2	2	...	1	...	14
April	...	...	...	...	4	2	1	...	...	...	...	7	2	1	...	...	...	..	...	...	3	..	1	...	...	1	6	4	1	...	...	...	...	...	11
May	3	2	1	6	5	3	...	1	...	...	...	9	...	1	...	1	...	...	...	...	2	1	...	...	...	1	9	6	1	2	...	...	...	..	18
June	1	1	...	2	4	1	...	...	...	..	...	5	3	1	3	...	...	...	...	1	8	1	2	...	...	3	9	5	3	...	...	...	.	1	18
July	...	...	...	...	10	3	6	1	1	1	...	22	6	1	...	..	...	...	1		8	6	2	...	...	8	22	6	6	1	1	1	1	...	38
August	...	...	...	...	17	15	2	2	...	...	..	36	18	9	3	3	...	1	...	...	34	6	1	...	...	7	41	25	5	5	. .	1	...	...	77
September	3	2	...	5	57	9	10	1	1	...	...	78	19	6	5	1	...	..	...	...	81	2	2	...	1	5	81	19	15	3	1	...	...	...	119
October	3	...	...	3	26	4	2	...	...	...	...	32	21	6	4	...	...	...	...	...	31	3	...	...	...	3	53	10	6	...	...	...	...	...	69
November	...	...	...	...	5	2	...	...	...	...	...	7	8	2	...	...	...	...	...	...	10	1	...	...	...	1	14	4	...	...	...	...	...		18
December	3	..	...	3	5	...	...	...	...	...	...	5	3	...	...	...	...	...	...	...	3	1	..	...	...	1	12	...	...	...	...	...	...	...	12
	13	5	1	19	137	40	22	7	4	1	1	212	87	29	15	6	...	1	1	1	140	22	9	1	1	34	260	83	39	14	4	2	2	1	405

* TABLE showing the ATTACKS in each Year after that in which the First Seizure took place, and the Month in which the first of these occurred.

	1830.				1831.								1832.							1833.				Total.								
	1 attack.	2 attacks.	3 attacks.	Total.	1 attack.	2 attacks.	3 attacks.	4 attacks.	5 attacks.	6 attacks.	7 attacks.	Total.	1 attack.	2 attacks.	3 attacks.	4 attacks.	5 attacks.	8 attacks.	Total.	1 attack.	2 attacks.	3 attacks.	Total.	1 attack.	2 attacks.	3 attacks.	4 attacks.	5 attacks.	6 attacks.	7 attacks.	8 attacks.	Total.
January	...	...	2	2	2	2	1	3	...	1	1	9	8	7	1	1	...	1	18	9	1	1	11	19	10	5	4	...	1	...	1	40
February	...	...	...	...	2	...	3	1	...	...	..	7	3	1	1	1	...	...	6	4	1	...	5	9	2	4	2	...	...	1	.	18
March	...	...	.	...	3	3	2	1	...	...	...	9	3	...	...	...	1	...	4	6	3	...	9	12	6	2	1	1	...	...	...	22
April	...	...	...	...	5	1	2	1	...	...	...	9	5	3	1	1	2	..	12	5	...	...	5	15	4	3	2	2	...	...	...	26
May	...	...	...	...	6	2	2	1	...	1	...	12	9	5	1	5	...	..	20	...	...	...	...	15	7	3	6	..	1	...	...	32
June	1	...	...	1	8	3	...	...	2	...	...	13	14	4	4	1	...	...	23	...	...	...	...	23	7	4	1	2	...	...	...	37
July	...	...	...	...	4	3	...	1	1	1	...	10	14	1	1	...	...	...	16	...	...	...	...	18	4	1	1	1	1	...	...	26
August	. .	...	1	1	11	4	4	...	2	...	...	21	13	3	...	1	...	...	17	...	...	...	...	24	7	5	1	2	...	...	.	39
September	1	1	...	2	14	8	3	...	...	..	...	25	9	...	...	1	...	...	10	...	...	...	...	24	9	3	1	...	...	...	...	37
October	1	1	...	2	4	4	...	1	...	..	...	9	9	2	...	. .	...	...	11	...	...	..	...	14	7	...	1	...	...	...	...	22
November	...	.	...	...	2	1	...	...	...	...	...	3	4	1	1	...	...	...	6	...	...	...	..	6	2	1	...	...	...	...	...	9
December	...	...	...	.	2	...	...	...		...	...	2	1	...	...	...	...	...	1	...	...	...	...	3	...	...	...	...	...	...	..	3
	3	2	3	8	63	31	17	9	5	3	1	129	92	27	10	11	3	1	144	24	5	1	30	182	65	31	20	8	3	1	1	311

it appears that in the first year of suffering from the Fever, in 260 individuals who had only one attack before the end of the year in which their original seizure took place, a gradual increase in the proportion of these to the whole cases is evident as the year passes on; and a diminution is at the same time seen in the extent to which the number of relapses occurred. By the same table, it will be observed that in one person eight attacks of the disease took place from the month of June to the end of the year: in 2 seven attacks; in the same number, six; in 4, five; in 14, four; in 39, three; and in 83 instances, two attacks occurred before the commencement of the year succeeding that in which the patients had been for the first time affected with Fever. In after years, again, it will be observed from the table given at page 102 that—setting aside the cases of January and February, in which the tendency to fever excited in the previous season, may be considered in some degree as still existing—the renewal of the morbid action took place in nearly the same circumstances as the original attack; being most frequent in the months of August and September of 1831, but occurring at an earlier period of 1832. The same remark in respect to the gradual diminution of the number of relapses as the year progressed, is applicable to these cases as to those of the former table; and, with regard to the recurrence of these in each individual, it will be observed, that in one person, from the month of January to the end of the year, eight attacks took place; in another, from February, seven attacks occurred; in 3, there were six attacks,—the first of these, in one person, not taking place till the month of July; in 8 persons, five attacks occurred; in 20, four; in 31, three; in 65, two; while of 311 persons, whose series of cases extended beyond the first year of their seizure with fever, the disease in 182 did not extend beyond one attack within the limits of each year.

On a general view of the attacks of individuals throughout the four years, it appears that the disease was confined to one seizure each in 119 persons, three of whom were affected in 1829; 49 in 1830; 49 in 1831; 17 in 1832; and one, until May 3, 1833. Again, in 54 persons, one attack occurred in two successive years; and in 17, the same took place in each of two years; but with one year intervening, in which the patient was free from fever. To 11 soldiers, the attack occurred once in each of three successive years; and two men were attacked once by fever in each of the four years, from 1830 inclusive. The disease was confined to one original attack and one relapse within the year of seizure, in 41 persons, of whom two were affected in 1829; 12 in 1830; 18 in 1831; and 9 in 1832; while two individuals were attacked by the disease twice in each of two successive years, and again, in another, after an interval of one year's immunity from the complaint. In 15 persons, the fever occurred once in the form of an original attack, and twice in that of a relapse in the year of seizure; but not at any other period; 13 of these individuals were affected in 1830 and 1831; and in the same years there were five soldiers who had four attacks within the year of seizure only; while another person had the same number within each of these two years, but not one at any other time. The remaining patients were attacked by the disease in more than one year, but in an unequal number of times in each. Thus, 32 persons were affected with fever in each of two years, mostly successive, the attacks being two in one year, and one in the other. In 14 persons the attacks, in like manner, were three in one year, and one in another, generally the following. In 31 persons, where the number of seizures altogether varied from five to twelve, they were confined to two successive years; while in 49 persons they were either distributed over three successive years, or, as in seven, there was one intervening year of immunity from the disease. In the same manner, a like number of attacks was extended in 13 persons over four years, with one exception, succeeding each other; while in one individual, who had nine attacks, these were distributed over all the period to which these observations refer."—Pp. 92–104.

The inquiry as to the causes of the disease appears, notwithstanding all the observations made of late years in India, to have furnished as little exact information as ever; and the result is that the causes of this disease are still little understood.

" The prevalence of Paroxysmal Fever appears intimately to depend upon the presence of cold and moisture; preceded and followed by intervals of heat. It does not seem very material whether, with many European medical writers, the aid of a malaria or with the natives of India, the morbific influence of impure water, be called in to assist the operation of alternations in temperature in producing these fevers; as the effect of either does not appear very sensibly when the air is warm and dry, and the range of the thermometer is within certain limits. The localities of fever in India may be divided into three classes: firstly, those situations in which the rains take place at an early part of the year, as from the month of June; secondly, those at a certain altitude; and thirdly, places surrounded by, or near to, extensive forests or jungles. The neighbourhood of hills, the existence of a soil retentive of moisture, and great exposure to certain winds, add to the intensity of the cause of fever in such situations; while the vicinity of the sea has, occasionally, the effect of neutralizing the influence of other circumstances in producing this disease. In all the above localities of fever, the same result is present at the period in which this complaint is most prevalent. In the first, when the ground has become saturated with moisture, should an interval of clear weather occur, with a powerful sun in the day-time and heavy dews at night—particularly if the rains of the previous year, or years, have been of a scanty nature—fever soon begins to prevail; and is more abundant in proportion to the duration, or temperature, of the preceding hot season, and the existence of the auxiliary causes of the disease above mentioned. In the cooler latitude of Kamptee, it is seen to have existed in greatest number in September; while at Cuddapah, its period of greatest prevalence did not take place until the coldest season; at which time few cases occurred at the former station. In both places, the rains had occurred in unusual abundance at the same time of year; but the feverish range of temperature took place at an earlier period in the cooler climate of Kamptee, than in that of Cuddapah, considerably to the southward, and notoriously one of the hottest stations in India. Again, in high-lying situations, as at Seringaptam, in latitude 12° 33′, but at the height of 2300 feet above the level of the sea, the greatest prevalence of fever is in the hot season, and seems independent of the fall of rain; while in jungly situations the only interval of immunity from the disease, throughout the year, is at this time. The temperature, therefore, in all these localities becomes reduced, at their most feverish season, to a certain point at one period of the 24 hours, by radiation from the earth, by the altitude of the place, or by the cooling influence of a wooded or hilly country; and a deposit of moisture takes place from the air at the same time. The usual increase of heat succeeds on the following day, and by one or more of these diurnal changes Paroxysmal Fever appears to become engendered. It does not seem that either a continued state of great heat, or of great cold, is productive of this disease. An alternation of temperature appears necessary to excite its phenomena; and the course of this change is observed to be, the occurrence of heat after that of cold, combined with moisture. It would be superfluous to show how the vicinty of hills, or the presence of moisture in the ground, tends to reduce the temperature at night to a greater degree than would otherwise be the case.

"It is sufficient to say, that in the former instance the cool air rushes down from the heights to occupy the place of that heated and elevated by the influence of a nearly vertical sun, thus bringing the temperature of the place to that of the surrounding hills; while in the latter, the solar heat of the day-time is expended in evaporating the moisture, rather than in heating the ground; and thus, as soon as the sun is withdrawn, a speedy reduction of temperature is the consequence. When either of these circumstances is in great degree, the influence of the vicinity to the sea-coast is scarcely felt in mitigating the prevalence of Paroxysmal Fever. This has sometimes been observed in the occurrence of severe attacks of the disease where hills and jungles have approached the sea-shore; but generally, fever is not frequent in such a locality; and, when it does occur, its paroxysmal nature is less marked than in more inland situations.

"It is to be remarked also, that great or sudden exposure to any extreme of temperature has often had the effect of exciting the phenomena of fever, either originally, or as a relapse. This has been frequently exemplified in the cases of individuals who have been seized with the disease after severe and prolonged exercise in the heat of the sun, with only slight shelter during the night; or in those of persons who have bathed in the cold streams issuing from the hills or jungles, and have afterwards been exposed to the ordinary heat of the climate, or—as is often the case—to exercise and exposure in the sun. Fever produced under such circumstances, particularly if an original attack, is often very severe and remittent in its nature, and has frequently proved fatal. When the disease is prevalent in a great degree, it has been observed that sheltered situations are most free from it; and that localities much exposed to the prevailing winds—whether from the west, combined with moisture, or from the east—present numerous cases. Occasionally, in houses placed on a rising ground, every individual has become subject to the disease in the course of a sickly season, and has had frequent relapses.

"Some auxiliary causes of an attack of Paroxysmal Fever have been observed, and where the disease has once taken place, these, of themselves, are often sufficient to produce a return. They seem to operate in the same manner as the extremes of temperature above referred to; one class increasing the action of the heart and arteries, and the other depressing it. In the former are included excess of food, or intoxicating liquors, violent exercise, or exposure to heat; the excitement produced by either of which often goes on to fever; while, in the latter, are found great evacuations; confinement to the house by other sickness; exposure to cold and wet; bathing in, or drinking profusely of, cold water and the like.

"The influence of the lunar changes has also been considered of some consequence in causing attacks of Paroxysmal Fever; but materials are not in the author's possession to exhibit the extent of this power in the body of Europeans to which these observations chefly refer. In the two native regiments, however, above alluded to, tables have been constructed with the view of showing the number of seizures with fever in each day of the moon; and one of these, including a period of 11 months, and referring to the cases at Seringapatam, is given below.* The facts contained in the other correspond in a great measure with those now exhibited; and the necessary inference is, that about the period of the full moon there is a slight increase of fever; but scarcely to such an extent as to warrant the belief of any direct power in producing this effect. The circumstance may be more easily explained by peculiarities of the weather at this period of the moon's age, or by the greater ex-

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* TABLE showing the number of seizures with FEVER on each of 29 days of the moon, during 11 lunar months.

On the 15th day	68 seizures.	On the 28th day	49 seizures.
... 14th ...	65 ...	... 2d ...	48 ...
... 11th ..	59 ...	... 6th ..	48 ...
... 16th ...	58 ...	... 13th ...	48 ...
... 20th ...	55 ...	... 21st ...	48 ...
.. 27th ...	54 ...	... 9th ...	47 ...
... 18th ...	53 ...	... 26th ...	46 ...
... 1st ...	51 ...	... 7th ..	45 ...
... 5th ...	51 ...	... 3d ...	43 ...
... 10th ...	51 ...	... 24th ...	42 ...
... 12th ...	51 ..	... 4th ...	39 ...
... 22d ...	50 ..	... 8th ...	39 ...
... 29th ...	50 ...	... 23d . .	36 ..
... 17th ...	49 ...	... 25th ..	31 ...
... 19th ...	49 ...		

posure to the night air, which the presence of the clear moonlight, when this luminary is near her full, gives rise to. It may be mentioned that, in the Madras European Regiment, there does not appear any increase or decrease in the daily admissions to hospital, corresponding to the intervals of the lunar changes; and it appears most probable, that the influence of the moon has been much overrated by those who have attributed a powerful effect on the prevalence of Paroxysmal Fever to the changes in this luminary."—Pp. 163—167.

Next in importance and frequency to fevers comes the tribe of affections of the brain. These the author distinguishes into four orders; first, Cephalic inflammation proper; second, Cephalic inflammation, or disease terminating in palsy; third, Encephalic or Cerebral or Meningeal disease with epileptiform symptoms; and, fourthly, *Delirium tremens.*

In the first of these forms of disease, though the history of the symptoms is given, no very precise account of the peculiar nature of the Cephalic inflammation is presented. But from the fact, that in a proportion of the cases quinine after local depletion was found either beneficial or necessary to the complete restoration of the patient, there is reason to think that several of the cases were paroxysmal fever masked or disguised under the aspect of cerebral or meningeal symptoms. It is well known that this is a frequent form of disguised ague in tropical regions and in miasmatic districts, and more especially under the influence of the equinoctial heat; and it seems certainly in every way probable that this was the main cause of the Cephalic inflammation here considered.

It must not be concealed, nevertheless, that various other remedial agents were employed, and that to their operation considerable influence in contributing to the favourable result is ascribed. Thus in ten cases the system was affected by mercury; in four the favourable effect was ascribed to the use of a blister; and in one to the application of leeches after salivation and the subsequent or concomitant use of a blister. In two cases only does the author ascribe the favourable result to the use of quinine.

In the first class of cases now referred to, the author thinks that no evidence was afforded of the existence of any previous or concomitant disorder of the nervous system, with the exception of six fatal cases in which serous fluid or vascular injection, or, as in one case, actual purulent matter was found in the brain or its membranes.

In another class of cases, however, amounting to six also, the indications of lesion of the nervous system evincing its presence by paralytic symptoms, became a prominent and conspicuous feature in the disorder.

In five cases, two of which occurred in the same individual, the palsy took place at the first admission of the patient to hospital, with corresponding pain in the head ; while in the two other patients, intervals of ten or fifteen months ensued, in the first case with three, in the other with four attacks of cephalic inflammation, before paralytic symptoms were observed. After this affection had taken place, the patient was also occasionally readmitted to hospital with symptoms connected with it, sometimes from aggravation of paralytic symptoms, but generally from a tendency to recurrence of inflammation. The number of cases referred to this head, including of course relapses and second attacks, amounts to twenty-two ; which the author proposes to distinguish, first, into those admissions taking place before the paralytic seizure ; secondly, those in which palsy presented itself ; and, thirdly, those cases showing the patient's history after that in which palsy had taken place.

The paralytic affection amounted to hemiplegia in all the instances ; the left side being paralysed in four cases, and the right in three.

In all these cases some amendment took place, though at different intervals. In one case an interval of twelve months elapsed before the disease was removed. In two cases the patients were unfit for duty at the period at which the author left the regiment In one, a case of relapse, the attack terminated fatally by convulsive symptoms. But no examination of the body took place.

In cases of this class, if, in the absence of positive information afforded by inspection, it is allowable to speculate on the state of the brain, it is reasonable to think that some morbid secretion or affection must have taken place in that part of the brain in which the origins of the locomotive nerves are found.

The causes specified as giving the tendency to cephalic disorders in India, viz. exposure to high temperature, atmospheric vicissitudes, intemperance, and the frequent abundant use of intoxicating liquors, seem to operate in producing a greater disposition to epileptic attacks than among the general population of a cooler and more temperate climate. The number of cases reported in the returns for July 1828 to July 1833, amounted to sixty-seven ; and during the four years of the author's personal charge, from May 1829 to May 1833, fifty-nine cases were reported to have occurred. Among fifty-six of these Dr Geddes draws the distinction between those cases, in which an epileptic habit appeared to have taken place, and those in which the attacks were either unfrequent, or consisted of spasmodic seizures without evincing any disposition to return. In the former case there is often organic, or at least, permanent disease of the brain or its membranes. In the latter the epileptic seizure may be the effect of temporary injection, congestion, or irritation.

The total amount of the individuals appears to have been 12; in one half of these the attacks were more or less multiplied; in the other half again, the individuals were admitted for single attacks.

It is clear that in both classes of cases Dr Geddes considered the epileptiform attacks, as either associated with, or dependent on the irritative action of inflammation. All the cases were treated more or less freely by general blood-letting, in some cases to a large amount, local blood-letting by means of leeches, the application of blisters, and the administration of purgatives; and, under certain circumstances, the use of opium, calomel, and quinine.

Delirium Tremens.—The fourth form under which cerebromeningeal disorder appeared took place in 27 persons; in two of these twice. In two the fatal termination took place.

In the whole 27 cases the use of intoxicating liquors for a certain number of days had preceded the attack of the characteristic delirium. In 16 cases the period elapsing from the cessation of drinking to the accession of delirium was ascertained. In one case the interval was 18 hours; in two, 24 hours; in two, 36 hours; in seven, 48 hours; and in two respectively, 60, 72, and 96 hours. This variety has no reference to the age or residence of the patients in India, or to the period of the year at which the disease manifested itself; nor had the author data to trace its connection with the nature of the intoxicating liquors employed, the length of time during which drinking had been maintained, or the sudden or gradual cessation of that process.

In six instances the delirium continued for 24 hours; in seven from 1 to 2 days; in three, from 2 to 3 days; in two, 4 days; in one, 5 days; in two, 7 days; in one, 8 days; in two, 11 days and 12 days.

This disorder seems to affect in India the older soldiers in greater proportion, than those of less age and shorter service. Of twenty-four subjects whose time of life was ascertained, the ages of six only were below 28 years, seven were aged 28 and 29, and eleven persons were beyond this age, and between 30 and 40. Of the six first mentioned, 3 were Irishmen, 2 English, and 1 Scottish; of the seven next specified, 4 were Irish and 3 English; and of the eleven last-mentioned as between 30 and 40, 7 were English, 2 Irish, and 2 Scottish.

The admissions were most numerous in the cold seasons, thirteen cases having been admitted in the months of October, November, December, and January; in the rainy months of June, July, August, and September, eleven cases took place, five of these being in July; and in the hot season, comprising February, March, April, and May, only five instances are recorded. This diversity in the prevalence of the disease at certain periods of the year, Dr Geddes thinks, is probably to be explained by the greater disposition to the use of intoxicating drinks at some times than at other periods.

The treatment was varied according to the nature of the case, the mode in which the symptoms had approached, and the subsequent effects. In simple cases opium was the great remedy. But in several (eleven cases at least) it was necessary to apply leeches to the temples; and purgatives were given in every case except three. In one case only was venesection employed, excepting those which terminated fatally. Calomel was given in several cases in large doses, sometimes so much as one scruple at a time. The author allows, however, that mercurial action had no influence in checking or moderating the delirium.

Of Thoracic inflammation, seventy-nine cases are recorded.

Of the chapter on Hepatic inflammation, great part of which was published in the sixth volume of the Transactions of the Medical and Physical Society of Calcutta in 1833, notice has been taken in its proper place in our 44th vol. p. 454, (1835.)

Rheumatism appears to be a common and frequent disease of the soldier in India, 457 cases having been admitted during the period of the author's charge, that is four years; or at the rate of 114 cases annually. Of these only 312 cases are available for reports; and among these, again, the individuals the subjects of the disorder were only 160, so that nearly half the number were second attacks or relapses.

The disease affected, in the great majority of cases, the bones or the periosteum and the joints; and, corresponding with this peculiarity, must be noticed another in the etiology of the disease, that many of the attacks are ascribed to the influence of syphilis alone, or the use of mercury, exhibited for the treatment of that distemper. This has made us entertain doubts whether these cases be rightly referred to the head of rheumatism. Without denying that rheumatism does affect the periosteum, we think that where the periosteum is inflamed, swelled, or painful, in consequence either of the secondary influence of the syphilitic poison, or the operation of mercury, that disease ought to be denominated and regarded as *periostitis*. We are aware that in common language it is often called rheumatism; but surely where it is attributable distinctly to the causes now mentioned, no advantage is to be derived either to pathology or therapeutics from viewing it as rheumatism.

It is quite clear from the author's account, that in many the periosteal disease was induced in the manner now mentioned. Thus he states that the periosteum was affected by the disease in various parts of the body,—as the *craninm*, *sternum*, collar-bones, ribs, *os sacrum*, *ulna*, *radius*, *tibia*, *fibula*, metacarpal and metacarsal bones, and fingers and toes. Now it is quite clear that, with regard to the whole of the bones specified here, from the skull to the *fibula* inclusive, these are the very parts of the osseous or rather periosteal texture, which are most commonly and most severely affected by chronic inflammation, the result sometimes of syphilis, sometimes of mercurial medicines given for the cure of that disease. In all cases, it is the periosteum of the bones nearest the surface which presents this peculiar tendency and disposition to thickening and painful swelling, with roughness, and not unfrequently exostosis and superficial caries of the bones.

Again, says Dr Geddes, the largest class of cases are those which owe their origin to a syphilitic source. In fifty-two individuals the rheumatic disorder was preceded at various intervals by a distemper apparently of this description. In thirteen persons the venereal affection had been confined to ulcers on the penis; in a like number there had been both ulcers and bubo, the latter, however, not proceeding to suppuration; and in seven the bubo or buboes had proceeded to this termination. In nineteen cases the disease had been confined to one or more buboes, without any ulcer having been observed or recorded during the patients' stay in the hospital, and in ten of such cases the glandular affection had advanced to suppuration.

In nineteen of these thirty-two patients, no mercury had been exhibited during the continuance of the venereal disorder; in four persons the medicine had been given but without affecting the mouth; in nine it had produced slight soreness of the gums; in eight a greater degree of this; and in twelve cases salivation had been considerable.

What was most wanted on this subject was to determine, whether in all, or any, or none of the cases in which the periosteum had been manifestly affected, syphilitic symptoms had existed, or mercury had been given. We think that upon careful inquiry, it would turn out that this had been the case. As to the circumstance of mercury not having been given in a certain number of cases during the existence, that is, we presume, for the cure, of venereal symptoms, that makes no valid objection whatever; for we have observed, that if mercury has been given at any previous time of the patient's life for the cure of any other disease, and if the individual so treated be subsequently attacked by venereal symptoms, then, either in consequence of the united action of the syphilitic poison and the mercury in the system, or from some other cause, periosteal inflammation of a chronic and obstinate character is very likely to ensue.

Dr Geddes, nevertheless, it must be allowed, takes care to say, that the facts did not shew the cases to be decidedly syphilitic, and while all that he stipulates for is, that in certain cases the syphilitic virus gives a tendency to the occurrence of rheumatic inflammation, he adds, that in referring the original attacks of such rheumatism to syphilis, it is not to be imagined, that the disorder is considered as of the same nature as those secondary symptoms of the venereal disease, which have been supposed in general to make their appearance from a deficient exhibition of mercury in a previous stage of the complaint. "The usually short period after syphilis in which the rheumatism shews itself, the absence of any other symptom of the venereal disease either occurring between the venereal disorder and the rheumatic affection, or at the same time as the latter, or subsequent to it, and the final cessation in many instances of the rheumatic attack without the aid of mercury, serve to shew that the cases of rheumatism here referred to are not to be considered as secondary symptoms of syphilis."—P. 459.

Certainly they cannot with justice be regarded as such. But it is a very unfortunate circumstance in this elaborate and careful view of the statistics of the rheumatic disorders of European soldiers in India, that no distinction of the kind already referred to is attempted, and that the periosteal rheumatism, as it is vaguely and loosely called, is here confounded with the other forms of that disease.

In all the classes in which the disease took place, its frequent recurrence or its lengthened duration, rendered the patient pale, emaciated, and lame; and other disorders were generated which obscured the original disease. The most common and formidable of these are stated to be ulcers on the skin and certain symptoms referred to hepatic disease. By either or both of these combined or succeeding each other, rheumatism occasionally proved fatal; and this result is stated to have taken place in seven cases.

On the subject either of causes or treatment, nothing very new is communicated.

The leading characters of this volume are great precision and accuracy. So far as conclusions deduced from a number of soldiers of the amount of the Madras European regiment, can be expected to give a higher degree of precision and certainty to our knowledge of the etiology and treatment of the diseases of India, the information afforded by the present work is calculated to contribute largely to that end. The author has manifestly bestowed great labour and pains in arranging and digesting his tabular researches, and interpreting their results; and this work must be referred to as a source of correct information on most questions relating to the diseases prevalent among Europeans in India.

In perusing the present volume, we have been deeply, forcibly, and painfully impressed by the pernicious consequences resulting to the health of Europeans, and especially European soldiers, by the excessive, habitual, and in a great degree authorized indulgence in the use of spirituous liquors. Though India, like all equinoctial regions, is evidently highly productive of disease to Europeans, and must always be naturally and necessarily so, it is farther manifest that the whole of the usual diseases are greatly aggravated in symptoms and oftimes rendered incurable, and that several other diseases are induced by this pernicious, and we may say, insane and fiendish habit. From the statements given by Dr Geddes, it appears that every European soldier is entitled to consume, in the course of the 24 hours, a certain amount of arrack,—a spirit as strong and potent as rum, and we presume in no degree less deleterious; that the greatest number do consume this amount, and sometimes a good deal more; and that this may be done under authority as a thing permitted or enforced. It appears farther that those who have the means of procuring them, and sometimes those who have not, consume in addition more or less brandy, gin, or rum; and it results, that this system of irritation and stimulation by intoxicating liquors is kept up from the first day of their arrival in India to the last of their residence in the country. Is it wonderful that under this systematic and regular course of poisoning, the sickliness and mortality of European troops in India should be very great, and entail upon their employers and the country at large an enormous expense? Is it not wonderful rather, that the troops are ever in a state of health and fit for duty? It is a demonstrated fact, that *delirium tremens* and several diseases of the brain are solely caused by the use of these liquors. It is not less certain that hepatic diseases, dysentery, and various affections of the brain, are very much aggravated by their use; that the disposition to all diseases is engendered and maintained by their use; that the persons thus drugged with spirituous liquors, neither stand medical treatment well, nor can be treated with safety or certainty; yet we read with distress and astonishment the decimation of European regiments, by disease and the total destruction, after six or seven years, of whole regiments in this climate.

The whole of this practice is carried on, it is to be observed, under the sanction and by the orders of government, and often with the example of the officers before the eyes of the men. How is it possible to expect that the soldiers should either preserve their health or conduct themselves properly under such treatment? When a regiment is encamped on a miasmatic piece of ground, the alluvial banks of a slowly flowing river, the borders of a swamp, or the neighbourhood of a dense impenetrable jungle, the men are attacked numerously by fever, and in some cases by dysentery and hepatitis. Of all this the reason is well understood. It is the telluric poison operating on the health of the men, and the measures calculated and known to counteract and neutralize its effects are promptly adopted and carried into effect. The corps are withdrawn if possible from the source of the poison.

Here, however, is a poison known and observed to produce most hurtful effects, allowed and encouraged to operate for years, carried about as an inseparable companion; and no means are taken to put a stop to the evil, though these means are much more completely within our power than the situation of an encampment, a bivouac on the banks of a malarial river, or the vicinity of a dense jungle.

Meanwhile the health of the troops is impaired; many men are in hospital, some with fever, some with dysentery, some with hepatitis, some with affections of the brain, and not a few with *delirium*, the direct effect of intoxication. Others are affected in health and disordered in mind, debauched and immoral, irascible, insubordinate, quarrelsome, vindictive, and wantonly brutal. Delinquencies increase, and even crimes are not unfrequent. The black-hole, fatigue-duty in an unhealthy climate, and the scourge are in frequent requisition. It is stated in these Illustrations, that two violent deaths occurred. One man in a fit of depression of mind succeeding intoxication shot another dead. The homicide himself, when sober again, was likewise put to death. Two lives are thus sacrificed and two soldiers lost to their country for arrack and rum, to the amount of about one or two rupees. Meanwhile many more are allowed to commit slow but certain suicide, and may be said in many instances to murder themselves without let or hindrance.

Let it be remembered that, by the men thus introduced, on arriving in India at the age of from 20 to 25, to a systematic course of drinking, intoxicating liquors were in their own country all but untasted. Most of them were agricultural or other labourers, who could merely procure bread and water, or milk at the best, and here they are put at once on a good diet of animal food, with an habitual allowance of ardent spirits.

It is farther to be observed, that all this is done among people calling themselves Christians;—enlightened, civilized, and intelligent; under a Christian government, who, though it must be allowed they have too long neglected the religious wants of their colonies, are now beginning to feel alive to the necessity of providing places of worship and religious instructors for their fellow-subjects in their Indian dominions. If all this is right, proper, and becoming a Christian government and people, we undertake to defend the morality of the slave trade, the merits of the practice of Suttee, the virtue of Thuggism, the infanticide of the Chinese, or any other enormity which the human race have perpetrated during the last six thousand years. European soldiers in India are first made slaves to strong liquors, and then they are punished for being slaves. They are instructed to keep themselves in health, and they are ordered or encouraged to use the most effectual and active agents for deranging health.

We are accustomed to consider ourselves as the most wise, pious, and exalted nation on the face of the globe, and in many respects the boast is not without foundation. We regard the Moslems, the disciples of the prophet, as ignorant, bigoted, prejudiced, and illiberal; as the thoughtless slaves of a system of blind fatalism, and the reckless propagators of a bloody, cruel, and merciless fanaticism. Did not Mahomet, nevertheless, act with wisdom of the most exalted order and the most benevolent tendency, when he forbid his disciples the use of wine and the fire-water of the Ghiaours. How much evil and mischief, how much disease both of body and mind has he spared his votaries by this judicious commandment? He doubtless thought that under the burning sun of Arabia, where his first converts were made, and in the south of Asia, to which he hoped to extend his doctrines, the use of such stimulants must have been hurtful in a peculiar degree. And so has it proved in the present day. Wherever the crescent is raised, and wherever the Koran is the rule of faith and practice, wine is refrained from, and water is the liquid by which the believer quenches his thirst; and the tribes and nations thus living are healthful, vigorous, and suffer little from disease. Wherever, on the other hand, the Christians have come to trade, to make money, to make war, or to rule, they have been unable to proceed without wine or spirituous liquors, and the consequence has been great disease, premature deaths, and a large proportion of mortality. If any one doubts the facts now stated, let him study the present volume and the Transactions of the Societies of Calcutta and Bombay.

Look, on the other hand, at the nations and tribes under the dominion of Islamism, how they have resisted this Promethean gift, and how several of them have thriven in consequence. At this moment, it is believed, and by many stated as fact, that the numbers under the sway of Islamism are greater than those under the rule of Christianity. The nations are populous, many of them powerful, all of them with attache, well-conducted subjects, prosperous in trade and wealthy; and if some have fallen before the superior power and resources of Europeans, it has been in consequence of internal causes of decay, treachery, misplaced ambition, avarice, and the unsettled state of government and society. It may safely be asserted, that of whatever prosperity they have enjoyed, the practice of abstaining from intoxicating liquors has been the main source, and that this habit of sobriety alone has counteracted almost all the other injurious tendencies of their religious belief and practice.

It has been said by some superficial observers, that soldiers endure better great fatigue, perform more easily arduous duties, and fight with greater courage, when supported by the stimulus of spirittuous liquors. All this has been disproved again and again, and it has been ascertained that, even in enduring fatigue, whatever excitement is immediately produced by their use is temporary and evanescent, and is followed by a large degree of depression. The duties performed, and the labours encountered under the excitement of spirituous liquors are indeed always much more fatiguing and weakening than if these stimulants were not employed. It is clear that were our soldiers and other Europeans to live as the Asiatics themselves do in this respect, with their other advantages, they would enjoy much better and more constant health, and would suffer less from disease and mortality.

On the other hand, it is well known that the Asiatics, and especially those under the sway of Islamism, were and are good soldiers, patient of fatigue, active, bold, and brave. Such were the subjects of the Mogul empire, and such has our own experience proved the Mahrattas, the Affghans, and other tribes to be. Neither did the Sikhs in our late encounter with them show themselves deficient either in strength, endurance, or courage; and they have been living for two hundred years under a modified Islamism.

It is therefore surely more than time for the British Government and British officers to get rid of this dreadful hallucination, this fearful error as to the use of spirituous liquors among the troops. If some persons, either officers or privates, shall persist in poisoning themselves in this manner, by all means let them do so as free-born Englishmen. But let it no longer be said that Government obliges the men to swallow daily and annually a certain amount of arrack or any other spirit, and then complain that the men are disobedient, insubordinate, irregular, or are in bad health, either in penal confinement or in the hospital. The experience of the evil effects of the system has been enough and more than enough to satisfy any candid person; and it is surely unnecessary to carry on an experiment so murderous any longer. We had first the experience of the West India Islands, then we have had that of India, next of China, and, lastly, of India again; and surely all this, with the proofs daily furnished by the navy and vessels on foreign stations, must be sufficient to convince the most obstinate defenders of the practice.

ART. III.—*The Sanative Influence of Climate.*—By Sir JAMES CLARK, Bart., M. D., F. R. S., Physician in Ordinary to the Queen and Prince Albert. Fourth Edition. London, 1846. Post 8vo. Pp. 412.

"THE expression climate, taken in its most general sense," says Baron Von Humboldt, "signifies all those states and changes of the atmosphere which sensibly affect our organs: temperature, humidity, variation of barometric pressure, a calm state of the air, or the effects of different winds, the amount of electric tension, the purity of the atmosphere, or its admixture with more or less deleterious exhalations, and lastly, the degree of habitual transparency of the air and serenity of the sky, which has an important influence not only on the developement of plants and the ripening of fruits, but also on the feelings and the whole mental disposition of man."

This definition is correct so far as it goes, but it seems to us deficient in comprehensiveness. Climate implies not merely states and changes of the atmosphere which sensibly, but also those which insensibly, or at least imperceptibly to us, or to our means of observation, affect our organs.

The operation and influence of climate as a means either of causing or curing disease has long been admitted; and all intelligent physicians have long agreed in recognising the beneficial effects of moving from a country where the weather and temperature are variable and liable to extreme vicissitudes, to one where they are more steady, subject to fewer irregularities, and where changes take place in such a manner that they may be guarded against or obviated.

The truth of all this is recognised. But the great problem both for the physician and the invalid, is to select the countries and districts, the climate of which is best suited to different diseases, and to different stages and periods of the same disease. It is not enough to send a patient from a country or district with a cold, moist, or variable climate to one in which the atmosphere or temperature is warm and dry, and in which the atmospheric changes are trifling or unfrequent. Patients may be and have been sent from cold or at least cool and temperate regions to those in which the air is warm or hot, and dry or hot, or it may be, moist; and their symptoms and sufferings, instead of being abated and mitigated, have become aggravated, until, so far as it is possible to form a comparative judgment of two states of circumstances not very easily compared, the progress of the disease has actually been accelerated, and the fatal termination has been anticipated.

Again, it has been observed that in certain stages of pulmonary diseases patients have been sent to warmer and it may be thought more genial climates, with the hope of having their symptoms alleviated, and the progress of their disease retarded, if not arrested. Yet it has been observed, that in not a few cases of this description, the sufferings of the patient have also been aggravated, and the onward progress of his disorder has been very much accelerated. This is mainly owing to the circumstance that for the stage of the disease the climate has not been well adapted.

For all these evils the great remedy is proper and careful distinction not only of diseases and their stages, but of climates supposed to be suitable, and careful and correct adaptation of the one to the other. In this way only can we expect to obtain from change of climate those real advantages which it is calculated to procure; and to eschew those evils which may arise from indiscriminate and reckless relegation of invalids to foreign lands and remote countries.

In studying to give this branch of practical medicine the philosophical and beneficial character now mentioned, no one has laboured more assiduously than the author of the present volume. He is perhaps the first author who perceived the indispensable necessity of the distinction and discrimination of diseased states of the human body in the employment of climate as a remedy, and the similar necessity of adaptation of the climate to the different diseases and stages of disease in which persons might be, who were desirous to try the effect of change of climate. This we conceive to be the prominent and valuable character of his work, and we are happy to observe that he continues in the present edition to keep this object steadily in view.

In close connection with this peculiarity we have farther to observe, that the work of Sir James Clark has been of very great service, indeed, in directing the attention of physicians to this part of the subject; and that his example has called into the field a number of able observers, who, impressed with the truth of the same principles, have in the accounts of various places as residences for invalids, studied to estimate the advantages and disadvantages of different localities in different diseased states, and have inculcated the necessity of the greatest attention in adapting the climate of different localities to the particular disease, or form, or stage of disease for which experience shewed that it was best suited.

In our thirty-second volume we gave an ample and minute account of the general contents of the work at its first appearance in 1829. And when the second edition appeared the following year, we briefly adverted, in our thirty-fourth volume, to the additions and changes which the author made. Since that time the work has gone through another edition, and the present is now the fourth.

The third edition, though containing all the materials of the first two, presented also much additional matter, and was farther almost written anew, so that it might be regarded as a new work. Perhaps, as with this third edition the present is more directly connected, we ought to premise what we now propose to say by some notice of the former. But as this has now been five years before the public, we must be satisfied with confining our present notice to the most recent only.

In the present edition, the principal addition made to the places of resort within the island for invalids, is the account of

Bournemouth, a small village on the Hampshire coast, nearly opposite to the western extremity of the Isle of Wight, from which it is distant about ten miles. This place, though situated close to, is not immediately upon the shore, as it has a range of undulating heights, varying from 50 to 150 feet above the level of the sea, placed between it and the beach. It is well protected from the north and north-east winds, but much exposed to the south westerly gales. The soil is composed chiefly of sand, but the author remarks that the ground would require to be well drained. It appears that there is at present little accommodation for strangers, but that the place affords great facilities for the erection of sheltered and commodious houses.

There are no records of temperature or other meteorological observations to enable the physician to determine the climatic peculiarities of Bournemouth. But from careful consideration of its position, its soil, and the surrounding country, Sir James Clark thinks, that it is impossible to doubt that Bournemouth deserves a place in the list of our best climates, and that, for a certain class of invalids capable of taking exercise in the open air, it affords a very favourable winter residence. If the winter temperature is lower and the daily range greater than at Undercliff and Torquay, and if Bournemouth is less protected from cold winds than either of these two places, it has the advantage over the former in the excellence of its roads, and the extent of open country around it for exercise, and it has an atmosphere of less relaxing and depressing effect than that of Torquay. The sea bathing ground is said to be good.

Branksome, a small village about two miles to the west of Bournemouth, is described as presenting a situation still more favourable to the invalid, in being more completely sheltered from cold winds.

Of the climate of Egypt, an interesting account is given, showing that residence in that country is extremely well suited to invalids who require a dry, warm, and steady temperature, during the European winter. The season at which Sir James Clark recommends invalids to visit Egypt extends from November to March; and from the facilities afforded by the Steam Navigation Company's vessels, the undertaking is one of comparative ease. In November the weather at Cairo is fine; and in this month the northerly or Etesian winds commence, and form a sort of prelude to the healthy season. During December, January, and February, this sort of weather continues with little interruption. The air is dry and elastic; the sky cloudless; rain rarely falls, and dew is seldom seen. The great, and almost only objection is, that while the days are dry and warm, sometimes oppressively so, the nights are extremely cold, especially immediately before sun-rise. This nocturnal cold may be and must be guarded against, in order to derive advantage from the salubrity of the climate. There is no doubt, for it has been long observed, that exposure to the night cold is a frequent cause of dysentery, ophthalmia, pneumonia, and other inflammatory disorders incident to the climate.

Prosper Alpinus, a physician of Padua, who had visited Egypt in the sixteenth century (1580, September), had observed this peculiar character of the Egyptian climate; and had noticed, not superficially, the influence they exerted on the production or prevention of disease, and on the health and longevity of the Egyptians.

Clot Bey, it appears, recommends the Egyptian climate to the consumptive, on the ground that consumption is a rare disease among the natives, and that among strangers who had come thither with symptoms of the disease he had seen many cured and others materially relieved. Nervous affections, including insanity are rare. Rheumatism is uncommon, and gout unknown.

Numerous inconveniences, nevertheless, and these not inconsiderable, beset the European traveller or sojourner in Egypt. The mode of travelling and living in boats, the only kind of abode after passing Cairo, is totally unsuited to those accustomed to the houses and apartments and comfortable beds of European countries. To females the undertaking is impracticable. Then the numerous vermin with which all places abound, both in houses and without, are a frightful source of affliction to Europeans. In short, unless Egypt were Europeanized, it is impossible to see how it can become a residence for European visitors and invalids.

Sir James Clark, nevertheless, thinks that it is well suited to those who are, or have been, long suffering from a deranged state of health, which scarcely admits of definition, and yet is well known as the result of sedentary habits, prolonged and anxious mental exertions, irregularity of living, and for those affected by chronic disorders of the respiratory passages, and those afflicted by chronic rheumatism.

Sir James Clark has further sketched a sort of course or route for the invalid to follow. Thus he recommends him to proceed to Egypt by the Mediterranean in October, to ascend the Nile in November, and get to Thebes by the end of that month or the beginning of December, and then make arrangements for passing the winter in Upper Egypt. In his way home in March or April he recommends him to visit Greece and Constantinople, which he may be ready to quit in May and arrive in England in the middle of June, before which time he thinks invalids who have wintered in a southern climate should not appear in England.

In short, invalids must in this respect be very much birds of passage; shunning the summer of the hot regions and the winters of the cold.

The greatest drawback to visiting Egypt with comfort or satisfaction is to be found in the present state of the quarantine laws. For though a vessel may arrive from a healthy port and a country where plague never prevails, there is no assurance that she may not be detained and made to undergo a quarantine of from 8 to 15 days or 21 days; and this after a voyage of four or five weeks becomes a serious tax on persons not in perfect health. The Egyptian authorities are so suspicious, that, if a vessel have on board one or two persons so feeble from chronic disease or sea-sickness, that they require to be carried on shore, quarantine is sure to be inflicted on the vessel and all it contains.

All this vexatious system, however, must undergo some change. The quarantine regulations ought to be entirely revised; and if all restrictions were abolished, it would produce much more beneficial and salutary effects than the present oppressive and arbitrary system, by which a few quarantine officers have it in their power to inflict great evil and suffering on inoffending travellers, and cause much pecuniary loss and delay to all parties.

Were this tremendous evil removed, and it can only be removed among the Egyptian government by the influence of English example,—Egypt would become a visitable country by the English. In the meantime it is to be feared that few will incur so great a hazard and so much annoyance as detention at a quarantine station implies, with the doubtful chance of recovery from a state of feeble health by subsequent residence or travelling in such a country.

There is, however, one circumstance that we think likely to be beneficial, even though the invalid does not choose to proceed to Upper Egypt. The voyage occupies each way from four to five or even six weeks; and this alone must be highly beneficial to various classes of invalids. The voyage homeward may be made the same or longer by stopping at various points, either on the Greek or the Italian coast; and in this manner sailing for 11 or 12 weeks may be insured in a good climate, on a sea not very tempestuous, and amidst scenes calculated better than any other in the globe, to awaken and maintain interest, and to occupy the mind agreeably and usefully.

Some additional information is communicated on Malta, and a good deal on Madeira. The former is mostly from the essay of Dr Sankey, which was published in this Journal when it first appeared. Madeira seems to obtain the largest proportion of Sir James Clark's praise among all the Atlantic Islands. It is quite possible that it deserves it. But we know no place to which a larger number of persons are sent, who are not benefited by their residence there. There is something wrong, we infer, in the adaptation of the stage of the disease to the climate, or patients are sent thither on improper medical authority, or without any medical authority at all.

The appendix contains, as in the third edition, an account of the climate of the continents and islands of the southern hemisphere, and some observations on the mineral waters of the continent.

In conclusion, we may repeat what we formerly said of this work, that the author has spared no pains to render it a useful guide to those who propose to change their climate for the recovery of health, and it should be very carefully studied by all those physicians who have occasion to give to invalids advice involving the necessity of wintering at a distance from home.

ART. IV.—1. *The Baths of Germany, considered with reference to their remedial efficacy in Chronic Diseases, with an Appendix on the Cold Water Cure.* By EDWIN LEE, Esq., Fellow of the Royal Medico-Chirurgical Society, &c. &c. Second Edition, considerably improved, with the Addition of General Remarks on Mineral Waters, and Notices of some of the French and Swiss Baths. London, 1843. 12mo. Pp. 268.

2. *Practical Observations on Mineral Waters and Baths, with Notices of some Continental Climates, and a Report (the Third) of the Cold Water Cure.* By EDWIN LEE, Fellow of the Royal Medico-Chirurgical Society, &c. &c. &c. London, 1846. 12mo. Pp. 134 and 42.

3. *A Description of the Mineral Springs of Aix-la-Chapelle and Borcette.* By L. WETZLAR, M.D. London, 1842. 12mo. Pp. 88.

THE employment of mineral waters for the cure of various chronic diseases, though long resorted to by various patients, and encouraged by a few physicians, has never in the British dominions been treated with that cordial and unqualified approbation which is accorded to remedies of undoubted and invariable efficacy. Most members of the profession have been in the habit of regarding these agents as possessing little or no power over the animal economy, and none over the presence, degree, or severity of disease; and where beneficial effects have manifestly followed the use of mineral waters, either external or internal, their effects have very generally, if not uniformly, been ascribed to change of air, change of scene, relaxation from business, freedom from anxiety, and, in short, to any thing but the supposed medicinal agency of the waters.

On the continent, and especially in Germany, it is very different.

The very powerful influence on the animal economy, and the presence, and amount, and symptoms of chronic disease is ascribed to the use of mineral waters, and the employment of these as agents in the cure of disease, is not only fully recognized in the practice of the healing art, but physicians devote great attention to the art of distinguishing and accommodating different mineral waters to particular diseases or sorts of diseases, and especially to certain forms and stages of disease. The example and doctrines of many of these German physicians have of late years exerted considerable influence on the physicians of other countries, and have at length obtained for the alleged virtues of mineral waters a more patient attention than they had previously received. To this result we think the excellent and philosophical treatise of the late Dr Meredith Gairdner largely contributed, in so far as that writer directed the attention of English physicians, to many circumstances previously overlooked in explaining the physiological operation and therapeutic effects of mineral waters.

Among those who have, since the publication of the treatise of Dr Gairdner, studied to make known to English physicians and English patients, the virtues and merits of the foreign mineral waters, and to explain the principles which ought to regulate their employment, Mr Lee, the author of the first two performances on our list, is entitled to a conspicuous place. Many of those mineral springs he has visited; at several he has resided for periods varying in length; and concerning all he has collected information, calculated to enable both physicians and invalids to form correct opinions on the therapeutic powers of these springs.

In the first of the treatises of Mr Lee, which is the most complete and comprehensive, the reader finds, after an introduction on the general characters and effects of mineral waters, a full and detailed account of all the principal mineral springs in Germany, whether for drinking or bathing, much useful information as to the best mode of using the waters, and observations on the forms of disease in which they are calculated to be most beneficial. This forms a valuable travelling companion and guide to persons proposing to visit any one of the German, or even of the continental baths, on which also, both in France and Switzerland, the author communicates much useful information.

The second treatise is more brief, and of a more general character, and is intended chiefly to explain shortly the principles on which mineral waters operate, the peculiar qualities which adapt them to particular morbid states, and the circumstances which render them admissible or inapplicable.

A short general view of both will enable our readers to form some judgment concerning the contents of these books, and their fitness for the purpose intended.

The employment of mineral waters is chiefly calculated for the relief or removal of chronic disorders. In these complaints, it is generally supposed that a very considerable change has been slowly and imperceptibly effected on the fluids and on the solids of the human body. Digestion has been perhaps long in an impaired, inactive, or disordered state. The circulation of the abdominal vascular system has become retarded, interrupted, and perverted. The excretions of the intestinal tube have not been regularly made or eliminated. The circulation of the portal veins is accordingly interrupted and stagnant, and a general state of abdominal plethora is gradually and slowly, but certainly established. The effect of this is manifold. In the healthy state, the chloride of sodium present in the blood is so decomposed as it were by something like an elective attraction and almost upon the electric or electro-magnetic principle, that the chlorine and sodium or soda are deposited in different parts of the alimentary canal, at different periods of the process of digestion. The vessels of the stomach, at one extremity of the pole, secrete chlorine; those of the liver at the other extremity, soda or sodium, which is united with the bile. In this morbid state, either little or no chlorine is secreted, or so little is formed in the stomach, that it is unable to dissolve the alimentary mass, which then undergoes fermentation, and acts like a poison along the whole tract of the canal. At the same time, a feeble bile, deficient in taurine and soda, is poured into the duodenum, and the proper changes are not effected on the alimentary mass in that viscus.

Of all this the effect is that the food is not properly digested; proper chyle is not prepared; the blood is not kept in a proper state to repair the waste of the different organs; and a disposition to various morbid states is created; and very often actual diseases are established.

Besides the derangement in the biliary secretion, the same imperfect action of the stomach is attended with or followed by a morbid condition of the urinary secretion. It becomes preternaturally acid; and uric acid or uric salts are formed; or it may become alkaline and the ammoniaco-magnesian phosphate may be formed. In this state of the system it is that gout, rheumatism, and rheumatic gout are formed; and in some instances accumulation in internal organs, more dangerous and hurtful than either take place.

It is chiefly in states of the system produced in the manner now described, and in the slowly formed and often imperfectly marked varieties of bad health which result from them, that mineral waters are useful. As the ingredients which they contain are often in small proportion to the mass of water, as, in short, they are very much diluted, and can act only when the dose is accumulated by long continuation, it is only by their being taken for a considerable time, that is, after many days or several weeks, that their effects on the economy can be produced. The present author, with many others, thinks that they must be digested. By this we believe, he merely understands, that they must be taken into the mass of blood by the lacteals and veins in order to produce any considerable or beneficial change.

Much of the efficacy of mineral springs depends on the state of intimate combination of the saline, metallic, and gaseous substances with the water; and much also on the particular temperature. The delicate and very minute doses in which the saline or mineral substances are given, enable them to enter the system, to penetrate its vessels without producing primary effects, and by operating slowly and gradually to effect great and decided changes on the textures and on the fluids.

The salts most usually found in mineral waters are the sulphates, muriates, and carbonates; less frequently nitrates, borates, and phosphates. The most common sulphate is the sulphate of soda or Glauber's salts, which exists in large quantity in some hot springs, as Carlsbad, and in smaller quantity in others, as Wiesbaden and Schwalbach. Sulphate of magnesia, or Epsom salt, is the next most common, and is found in the waters of Wiesbaden, Aix-la-Chapelle, Schwalbach, Kissingen, Marienbad, and one or two more. When it predominates, which it does in certain cold springs, as Saydschutz, one pint containing eighty grains of salt, these waters are named bitter-waters.

Of the muriates the most common and generally the most abundant are muriate of soda, or chloride of sodium (common salt), both in hot and cold springs. Thus it forms a large proportion of the saline matter in the hot springs of Wiesbaden, those of Baden-Baden, Aix-la-Chapelle, and in the cold springs of Homburg, and Kissingen, Kronthal, and Kreutznach; while it makes but a small part of the saline ingredients in the waters of Ems, Schwalbach, Spa, Marienbad, and Franzenbad.

Carbonates are also common in saline springs. The carbonate of soda is the characteristic ingredient of many alkaline springs, as those of Ems, Vichy, and Bilin. In several instances, indeed, much carbonic acid is united with lime, as in Wiesbaden, Ems, Schwalbach, Kissingen, Marienbad, and Homburg, or with magnesia, as in all these waters in like manner. In short, where carbonic acid is present, there is generally both lime and magnesia, and sometimes iron.

There is strong reason to believe, that in the case of the whole of the springs of this water, the lime and magnesia are at first separated in the pure or caustic form, and are united with carbonic acid in excess at first; and that gradually, as they are exposed to the atmosphere, their super-carbonates are rendered neutral, and then deposited as calcareous incrustations. Certain it is, that on the surface of most of these waters, after they have been sometime exposed to the atmosphere, there is formed a thin opaque pellicle of lime or magnesia, and both are deposited from waters of this class.

Wherever springs of this order are cold, they are also sparkling and slightly acidulous from the quantity of carbonic acid gas.

It appears to us, and has long done so, that all the analyses of these mineral springs present the ingredients in a much greater degree of complexity, and in much greater multiplicity than they actually exist in the water when in solution. Thus to take the analyses of Wiesbaden and Aix-la-Chapelle waters, the statement gives not fewer than eleven saline ingredients in each. It is nevertheless probable, if not certain, from the ingenious observations of Dr John Murray of this city, in his Analysis of the Dunblane waters, that these eleven ingredients could scarcely exist chemically in the water, however they may be obtained as results of the analytic processes. Thus let us take the Wiesbaden water, which is said to contain the following ingredients in their respective proportions, and see what is the inference to be deduced. The analysis is that of Kaestner.

	Grains.
Muriate of soda,	44·225
Sulphate of soda,	0·700
Muriate of lime,	5·480
Sulphate of lime,	0·420
Carbonate of lime,	1·660
Muriate of magnesia,	0·790
Carbonate of magnesia,	0·700
Extractive matter,	1·750
Iron,	0·078
Muriate of potass,	1·200
Fluate of magnesia,	1·600
Total,	57·593
Carbonic acid gas,	7·166 cubic inches.

Now in this analytical statement are four acids, hydrochloric, sulphate, carbonic, and fluoric; two alkalies, one in large proportion, soda, the other in small proportion, potass; two earths, lime, and magnesia; and one metal in very small proportion.

Now it is most likely that these acids, alkalies, and earths would arrange themselves, and do arrange themselves, in the following manner. The hydrochloric acid is most likely combined with the soda, forming chloride of sodium; the sulphuric acid with the magnesia, forming Epsom salt; while the carbonic acid is in all probability united with the lime. There may be some trace of muriatic acid and lime, but it seems not necessary; and it is quite as likely that it is the result of the analytic processes. As to the fluoric acid, it may be joined with lime, and most probably is so; as we know that substance is much more common than fluate of magnesia. This view would present the following arrangement of the analysis:—

1. Muriate of soda.
2. Sulphate of magnesia.
3. Carbonate of lime.
4. Fluate of lime.
5. Carbonate of potass.
6. Iron.
7. Extractive matter.

By the same process the eleven ingredients of the Aix-la-Chapelle waters might be reduced to 6 or 7 substances, viz.

Sulphur,
Muriate of soda,
Sulphate of magnesia,
Carbonate of lime,
Phosphate of lime,
Fluate of lime,
Silica and carbonate of strontian.

With regard to fluoric acid, it is a curious fact that it has been shown by an ingenious chemical analyst of this city to exist in a great many waters which were not supposed to contain it, so that it is not wonderful that it is found in mineral springs.

The temperature of mineral springs is not a less important element in estimating their physiological and therapeutic effects. Many have asserted that waters heated by artificial means exert on the economy effects equally striking and decided as the natural thermal springs. This, however, is as confidently denied by all the German and many Foreign physicians; and it is alleged that whereas the use of baths artificially heated is followed by great relaxation, and considerable loss of strength if long continued, these natural hot waters produce no debilitating effects of this kind. Making every allowance for the exaggerations natural to advocates and defenders of any set of doctrines, and the depreciation of their good effects by opponents, it seems probable, if not certain, that there is some truth in the statement, that natural thermal springs may be used both internally and externally, if with due precaution, with less chance of injury than waters artificially heated.

It is said even that they retain their high temperature much longer than waters artificially heated to the same degree. This, however, has been represented on the authority of the experiments of MM. Longchamp and Anglada to be a mistake; and it has been again asserted that, while the slow process of cooling is common to mineral waters and saline solutions, the opinion of the particular nature of the caloric of thermal springs, and of their power of retaining heat for a longer period than ordinary waters, is a mistake. Though to this conclusion Mr Lee is inclined, he is disposed, nevertheless, to think that the temperature of a thermal spring must materially modify its operation in the human body.

It is foreign to our object to inquire into the source of this elevated temperature in thermal mineral springs; and perhaps this is of less moment, as nothing very satisfactory is known on the subject. Many have been ascribed to volcanic agency; and perhaps the volcanic appearances of the country in which they are situated, and not uncommonly the crateriform structure of the spot or spots from which they issue, may favour this notion. But this is rather at variance with a fact stated by Von Humboldt, that the hottest permanent springs yet known are at a distance from any volcano,—the Aquas Calientes de las Trincheras in South America, between Porto Cabello and New Valencia, and the Aquas de Comangillas in the Mexican Territory near Guanaxato. The first of these had a temperature of 90° 3′ Cent.=194° 5′ F., and issued in granite; the latter, which issued in basalt, had a temperature of 96° 4′ Cent.=205° 5′ Fahr., a few degrees below boiling water.

It was remarked by Arago in 1821, that the water of the deepest Artesian wells was also the warmest; and the truth of this remark has been confirmed by subsequent observations made in boring the deep-seated strata of the earth; the temperature having been found to increase at a certain rate for every increase in depth. Daubuisson also has shown, by comparing the temperature of different mines, that the temperature increases as we descend to greater depths. These facts have led all the most able observers to ascribe the high temperature of thermal springs to the high temperature which is found to be possessed by the deep-seated strata; and so firmly established does this doctrine appear to be, that it has been thought that it is possible to estimate the depth at which any given mineral spring issues, by observing its temperature. "According to our present knowledge," says Von Humboldt, "of the increase of heat at increasing depths, the strata by contact with which these high temperatures of the Aquas Calientes de las Trincheras, viz. 194° 5′ Fahrenheit, and the Aquas de Comangillas, viz. 205° 5′ Fahrenheit, were acquired, are probably situated at a depth of about 7800 English feet, or above two geographical miles."*

The knowledge of this circumstance, however, is of little moment to the therapeutic application of these waters; and we may be satisfied with understanding that the more elevated the temperature of a mineral spring, from the greater depth, there is reason to believe, does it proceed.

The temperature of a mineral spring is an important element in determining its therapeutic application. Not only does the increased temperature act as a medicinal agent, whether the water be internally used or externally applied, but it aids and facilitates the operation of the saline or other ingredients. The increased temperature is in the alkaline waters of Ems and Vichy a powerful adjuvant to their operation ; it is not less so to the saline and calcareous waters of Wiesbaden; and to the beneficial influence of the sulphurous and saline waters of Aix-la-Chapelle and Borcette, it appears to contribute much. This elevated temperature it is which particularly adapts mineral springs to be used as baths in gout, rheumatic disorders, cutaneous affections, diseases of the joints, and affections of the urinary organs. In affections of the brain and spinal marrow, and in those of the heart, and several of those of the lungs, their use is contraindicated.

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* Kosmos, p. 208.

Whatever be the opinion, nevertheless, entertained regarding the actual arrangement of these acids, alkalies, and earths in mineral springs, it is impossible not to agree with the author, that springs so impregnated are to be considered as strictly alterative remedies; that is, remedies not producing uniformly at least an immediate effect, but by being taken into the system, influencing all the secretions and excretions, and producing finally decided and remarkable changes in the functions. Some, as the chalybeate and alkaline waters, produce their effects independently of any increase of secretion. Others even of reputed laxative qualities, operate at first by producing constipation and suppression of the intestinal and bilious secretion. During the employment of certain waters, as several of the chalybeate, the hydro-sulphurous, and the saline, an assemblage of symptoms denominated bath-fever is not uncommonly induced. Dr Kreysig says, more particularly in reference to the Karlsbad waters, that a cure is often preceded by a state of indisposition with retarded or diminished excretions, showing, it is said, that the fluids are saturated with the mineral water; and when the fluids are thus charged with the principles of the water, a tendency to critical evacuation is produced.

> " The blood," he says, " presents the most manifest signs of vital expansion and increased vital tension; the face becomes red and swelled, the pulse strong, the sleep agitated and interrupted. There is frequently a sense of weight in the limbs, with disinclination to muscular exertion. The belly is swelled if there be constipation ; the chest is highly oppressed, and the head is the seat of a sense of weight and aching. The alvine evacuations are frequently suppressed, even in persons without disposition to constipation. After a fortnight or three weeks, sometimes later, a crisis suddenly takes place by evacuations from the bowels, followed by prompt and general relief. From this time the water continues to cause moderate evacuations, and to exert a salutary influence both on the local disease and the general condition of the patient."
>
> " Others do not attain this point during the treatment, but, on the

contrary, find themselves worse, and two or three weeks afterwards a change occurs, followed by abundant alvine evacuations, and the consequent cure or relief of the patient."

"Again, other patients," says the same author, "experience no alteration in their symptoms, neither during nor immediately after the treatment. Such persons should be subjected during the winter to treatment by medicines, which have an analogous operation to the waters. The cure is frequently obtained in this manner, or at least is prepared for next season, when, on resuming the use of the same waters, they are observed to produce a speedy effect, and cure as if by enchantment."

To the same effect is the opinion of Dr Von Ammon.

"Mineral waters," says this author, "can only produce their beneficial effects in a slow and gradual manner. A hurried perturbatory employment of them not only prevents the cure, but almost always causes an aggravation of the disease. They are drunk, in order to become mixed with the blood by means of the digestive powers, and in this manner the curative changes on the body are effected."

"Cold waters are digested more slowly, and their operation on the vascular system is less penetrating than warm mineral waters. Hence critical signs are much more frequently induced by the use of the latter than the former."

The beneficial operation of a course of mineral waters is not always evident at the time at which the patient is taking them. The system requires to be charged with them and their principles; and the time which may elapse before manifest effects appear, varies in different individuals. The waters, then, after an interval of weeks, sometimes begin to produce slow, gradual, gentle effects on the secretions and excretions; and, as these are acted on, the health of the patient is improved, and the local disease, under which he suffered, is either much relieved, or altogether removed. This subsequent or secondary operation of mineral water, the German physicians have been in the habit of calling the after operation (Nachwirkung). Sometimes even this subsequent operation does not take place, or is not observed. In most instances, it shows itself in some way or other. It is believed by Von Ammon, that many diseases are too inveterate to be affected by even a four or six weeks' course of bathing and drinking the waters. The rule, then, is to intermit them for a time, and recur to them again. In others, the symptoms are immediately aggravated, and the patient seems worse. Eventually, however, this aggravation disappears, and he feels better from the use of the waters.

A course of mineral waters, or cure, as it is termed in Germany, lasts from four to six weeks, though there are instances in which two or three weeks produce decided and manifest effects. Some waters, as the cold saline, or cold chalybeate, are used only in the way of drinking. Others are used only, or chiefly, as baths;

but many, especially of the hot saline and alkaline springs, are employed both as baths and internally. In some instances, cold waters are heated artificially, in order to be used as baths; but, as the increased temperature makes a decided change on the chemical composition, the propriety of this course may be doubted.

Mr Lee distinguishes mineral springs in the following order: 1. Sulphurous, or those containing sulphur with saline ingredients, as those of Aix-la-Chapelle, Aix les Bains, and the waters of the Pyrenees, among hot springs, and Weilbach and Harrogate among cold springs. 2. Chalybeates, or those containing iron. 3. Saline mineral springs, or those containing muriate of soda, as Wiesbaden, Boubonne les Bains, and Borcette. 4. Saline aperient springs, or those containing sulphate of soda (Glauber's salt) or, sulphate of magnesia (Epsom salt), or both. 5. Cold salt springs (Soolquellen), as Salzhausen, Kreutznach, and Kissingen. 6. Alkaline springs, as Vichy, Ems, and Toeplitz, among the hot springs, and Bilin, Fachingen, and Selters among the cold. 7. Acidulous springs, or those containing abundance of carbonic acid, as Selter and the Maxbrunnen at Kissingen; and, 8. highly mineralized springs, or those which, scarcely tepid, contain a small proportion of saline or other constituents, of which Buxton, Gasta, Wildbad, and Pfeffers, are given as examples.

We do not propose to enter into the detailed medical history, or application of all these baths at present. But we shall, in order to illustrate the general history of mineral waters, notice two, Aix-la-Chapelle and Wiesbaden.

Aachen as the Germans call it, or Aix-la-Chapelle, as it is named by the French, is a place of high antiquity, as the seat of a mineral water. The town is situated at the base of a chain of hills, of moderate elevation, in ground occupied by the ancient forest of Ardennes. It is about 29 miles distant from Vervier, 40 from Liege, and 43 from Cologne and the Rhine; and stands on the north of the Belgian-Rhenish Railroad, from which it is seen at the distance of about one mile and a half.

It possesses six springs, three superior, and three inferior springs. The source dé l' Empereur is the hottest and most impregnated with sulphur. Its temperature is 135° Fahrenheit. It supplies the Fontaine Elise the great drinking fountain, the Bain Neuf, the Bain l'Empereur, and the bath of the Queen of Hungary. Another of the superior springs is the source St Quirin, which rises in the bather's house of the same name. The temperature of this is 115°.

The inferior springs are distinguished from the superior by having a lower temperature, the heat not being more than 112°, and accordingly being less stimulating. These are termed the Source de la Rose, Source St Cornelius, Source St Charles. The advantage of these inferior springs, which supply one drinking fountain, and several bathing houses, is that they may be used in cases in which the stimulating properties of the superior would be hurtful, or at least might disagree.

The waters of Aix-la-Chapelle being hot, saline, and sulphurous, are highly exciting. Used in the form of bath their action is penetrating and stimulating, producing diaphoresis and relaxation of the system. They are liable to cause determination of blood to the head and lungs in persons thereto disposed, and in any disease or morbid condition of these organs their use is contraindicated. Taken internally the water is alterative, sometimes aperient, affecting mostly the lower intestines.

The diseased states of the system in which these waters are most serviceable, are derangement of the alimentary canal with abdominal plethora, gout, rheumatism, and periosteal pains. Several of the German physicians regard the waters of Aix-la-Chapelle as almost specific in chronic rheumatism, and superior to those of Wiesbaden. In contractions of the joints with lameness they are said to operate with much benefit; though it is usual for patients to employ the assistance of rubbers and champooers, who are said, at this watering place, to be more dexterous and successful than anywhere else on the continent. The disease, in which these waters appear to be really most useful, is chronic periostitis with roughening and elevation of the surface of the bones and periosteum. Their effect is to alleviate pain and apparently suspend the progress of the chronic congestion. They do not, however, always remove the swellings consequent on this disease.

In those persons also whose systems have been saturated, and whose constitutions have been impaired by copious and repeated courses of mercury, more especially for the removal of symptoms reputed venereal, the use of the Aachen waters is most beneficial. Affections of the periosteum, and bones, ulcers of the skin, ulceration of the throat, and other secondary symptoms, all either derive much relief, or are finally cured by the judicious use of these waters.

Close on the railway station lies the village or town of Borcette, in German, Burtscheid, which is provided also with hot springs, not very dissimilar to those of Aix-la-Chapelle. These springs are also distinguished into superior and inferior. The superior, however, differ from those of Aachen by not being sulphurous; and as their temperature is high, 150° Fahrenheit, and they contain a large quantity of muriate of soda, with small quantities of carbonate of soda and other salts, they resemble in some respects the waters of Wiesbaden.

The inferior springs of Borcette resemble those of Aix-la-Chapelle in containing sulphur, though in smaller quantity. These waters are generally less heating than those of Aix. It is further no small advantage to have in the same town saline and sulphurous waters, which the patient may use together or separately, according to their effects and the directions of the physician.

These waters are adapted for the treatment of the same diseases as those of Aachen.

On the right bank of the Rhine, about 70 miles above Cologne, the hills begin to diminish in height, and the ground is less mountainous. But a range of hills of moderate elevation runs into the interior of the country towards the east, then bends round in a southern direction, and is connected with the chain in which the Melibocus makes a conspicuous figure. This chain of hills, which was known by the Romans under the name of Taunus, and is named Heyrich or Hoehe by the modern Germans, encloses a sort of basin or hollow, varying in diameter from 10 to 20 miles, and very irregular in shape, but forming a very extensive and sheltered valley, on the right bank of the Rhine, and between that river and the Maine. In this hollow is situate Wiesbaden, about 3 miles from the Rhine.

The town is partly old and partly new, many new streets having been laid out, and many houses erected at its southern and eastern border during the last 20 years.

There are said to be four springs in this place; the Kochbrunnen, which is the principal, and perhaps the main source of the whole; the Adler or Eagle Hotel spring; the Four Seasons, at the hotel of the same name, and the Schutzen Hof, also at the Schutzen Hotel. The temperature of the Kochbrunnen or boiling fountain is 158°; and that of the Eagle is nearly the same. The temperature of the Four Seasons' Hotel spring is lower; probably from being at a little distance from the Kochbrunnen. That of the Schutzenhof we have had no opportunity of examining. It is said by Mr Lee to be about 15° lower than the Kochbrunnen, that is, about 143°.

This difference is, however, of little moment, as even the coolest is too hot for immediate use; and the waters of all, when employed for bathing, are allowed to cool for several hours.

They are always cooled also previous to being drank.

The composition of these waters has been noticed already. Used as baths they are detergent, softening, penetrating, and diaphoretic. Internally they have seldom much effect, except on the urine, which they increase in quantity, and which they render alkaline.

The diseases for the cure of which the Wiesbaden springs are supposed to be most adapted, are gout, chronic rheumatism, rheumatic gout, neuralgia, and abdominal plethora.

In gout, it is said the more inveterate the disease is, the more likely are the waters to prove efficacious. This is one of those absurd paradoxes, which none but a knave would utter, and very few but fools would believe. It is contradicted by the daily experience of physicans and patients at Wiesbaden. In all cases of gout the waters are not equally safe or equally serviceable. Wherever there are acute symptoms, and wherever the bowels are constipated, the waters do no good. In cases of long standing, however, with lameness from stiffness of the joints, the waters are occasionally beneficial. Occasionally in the atonic gout, the use of the waters, external and internal, is followed by an acute attack. They should then be intermitted for several days, while the patient is subjected to the use of medicine and regulated diet, rather low.

The waters may afterwards be resumed; and after this are resumed with advantage.

There are, nevertheless, many cases of chronic gout, with lameness and rigidity of the joints, or calcareous deposits around them, in which the Wiesbaden waters are of no use. The cases in which they are most beneficial are those in which two or three attacks have taken place, and left the patient more or less infirm and debilitated. The use of the waters for a few weeks generally relieves present pain, and diminishes stiffness; and either mitigates the next attack or wards it off entirely.

In chronic rheumatic disorders the Wiesbaden waters have enjoyed, with some physicians, high reputation. This is not in all cases well founded. There is no doubt that a certain class and form of rheumatic affections are relieved, and sometimes cured. Among these are sciatica, lumbago, general rheumatic feelings in the limbs. It is also said that articular rheumatism is cured by it; and so sometimes it is. But there are many cases in which, whether from disorganization within the joints, or other causes no relief ensues; and, in some instances, the disease seems aggravated. Whether from this cause or not, it is certain that various respectable physicians in Germany recommend for the cure of rheumatism, not Wiesbaden, but Aix-la-Chapelle; and others recommend the baths of the Pyrenees.

The present author states that, in the rheumatic disorders incident to military men on duty, the Wiesbaden waters are of sovereign and certain efficacy.

In many rheumatic disorders and neuralgic affections connected with a deranged state of the digestive organs, the Wiesbaden waters may be employed with great benefit.

They are also recommended in paralytic cases. But the wisdom of this seems questionable.

Many other topics we should have also noticed in speaking of these volumes. But our limited space warns us that we must make a conclusion. On some future occasion we may have an opportunity of again directing the attention of our readers to this subject.

PART III.

MEDICAL INTELLIGENCE.

ANIMAL CHEMISTRY AND MATERIA MEDICA.

On Food and its Principles. (Dr R. DUNDAS THOMSON's Experimental Researches on the Food of Animals, 1846.) — That the systems of animals are capable of sustentation by a supply of fibrinous matter almost alone is obvious from the history of the primitive inhabitants of the prairies of America. It is stated on good authority (Catlin), that there are 250,000 Indians who live almost exclusively on buffalo flesh during the year. The fresh meat is cut in slices of half an inch in thickness across the grain, so as to have fat and lean in layers, and is hung up exposed to the sun and dried. Upon this food, which is pounded, and eaten sometimes with marrow, the wild hordes of the West are not only nourished, but it is obvious that the heat of their bodies is kept up, since they taste no vegetable food whatever. Fibrin, then, is calorifiant, or capable alone, we infer, of producing animal heat. Liebig, it is well known, divides the function of the food into nutritive and respiratory. I have ventured to employ, instead of the latter term, the expression calorifiant or heat-producing, so as to give a wider range through the whole system to the functions of the unazotised food than the more local term of respiratory would appear to imply. According to this view all food is destined for repairing the waste of the body, and for the production of animal heat. The heat may be produced by the union of the carbon and hydrogen of the food with oxygen (the latter gaining admission to the system by the lungs, stomach, and skin), or by the condensation of oxygen during its substitution for hydrogen and formation of oxygen products. The preceding inference we also deduce from the experiment in which a dog was fed for some weeks on the glutinous matter of flour (Magendie); and it may be further concluded, that fibrinous or albuminous matter when given alone, is partially converted into carbonic acid, and is removed from the system during the process of expiration. But it would appear, from consideration of the experiments which have been made on the nutrition of animals with pure fibrin, that an auxiliary in the production of animal heat is either indispensable or highly advantageous, since animals fed on fibrin alone invariably declined in health (Magendie), and the American Indians have a certain admixture of fat with their dry meat, and are in the habit likewise of using marrow with it.

The reason why an auxiliary is required for the supply of animal heat appears to be, that the fibrinous matter which is taken up by the vessels of the intestines and is carried into the blood, requires to pass hrough the condition of muscular tissue before it can be of service as a calorifiant agent. The only view which appears at present to be tenable is, that all or the greater part of the fibrinous and albuminous matters which enter the blood displace a certain amount of the same substances existing in a solid form in the system, as brain, muscle, &c., and that the displaced matter undergoes certain modifications; probably, for example, it passes into the form of gelatin, and is excreted in the soluble state of urea, uric acid, and nitrogenous products. It is in passing into these last conditions that we can alone expect fibrinous matter to give out animal heat. Time therefore is required to produce these changes. It is to save the system, then, from too rapid waste, and at the same time to afford an abundant supply of heat, that the calorifiant food is required, and is always employed by all members of the human family who have advanced beyond the savage state.

That the amount of calorifiant food, in contradistinction to nutritive food, properly so called, as it has been well defined by Liebig, is out of all proportion greater than that required to supply the waste of the solid matter of the body, is obvious from the following table, which represents the amount of the ultimate constituents of the food of a stall-fed cow, consumed during one day:—

	Food.	Fæces.	Consumption.
Carbon . .	11·90 lbs.	5·10 lbs.	6·80 lbs.
Hydrogen . .	1·61	0·62	0·99
Nitrogen . .	0·45	0·20	0·25
Oxygen . .	10·74	4.12	6·62
Ash . .	1·71	1·09	0·62
	26·41	11·13	15·28

The food in this case was grass (the Lolium perenne, or rye-grass). If we now calculate the amount of food which was destined for nutrition by the formulæ below,[*] we find that it amounts only to 1·56 lbs. as represented in a tabular form.

	Nutritive.	Calorifiant.
Carbon . .	0·828 lbs.	5·982 lbs.
Hydrogen . .	0·109	0·771
Nitrogen . .	0·250	
Oxygen . .	0·373	6·247
	1·560	13·000

A true system of dieting would therefore require such tables for each condition of animals, in order that a comparison may be instituted between the wants of the system and the food. If this mode of viewing the question be correct, then the relation of the nutritive part of the food absorbed by the animal system in the preceding experiment is to the calorifiant portion as 1 to 8⅓ nearly. By comparing this fact, then, (which is independent of all hypothesis) with the different varieties of human food, it is probable that some light may be obtained in reference to the differences in the relative proportion of these constituents. Milk, for example, the food of the infant mammalia, contains one part of nutritive to two parts of calorifiant constituents, and in the growing state of an animal the nutritive part of the food not only supplies the place of the metamorphosed solids, but an additional amount of it is required to increase the bulk of the individual; and, as we have already stated that animal heat is generated by the change or degradation of the fibrinous tissues, it is obvious that in the nourishment of infant life there is a supply of heat from the casein, vastly superior to that afforded by fibrin supplied to full-grown animals, because the amount taken in proportion to the quantity of calorifiant matter is much greater. If we refer, again, to the food which is generally employed by the inhabitants of this country, wheat and barley, we find by a mean of experiments afterwards to be detailed, that the average amount of albuminous matter present in them is 11 per cent., while the quantity of starch and sugar existing in these substances may vary from 70 to 80 per cent.; thus affording the proportion of nutritive to calorifiant food as 1 to 7, and upwards. Such food, it may be inferred, is fitted for the consumption of an animal which is not subjected to much exercise of the muscular system, and may be viewed as the limit of excess of the calorifiant over the nutritive constituents of food. As the demands upon the muscular part of the frame become more urgent, the proportion of the azotised or nutritive constituents should be increased, and this may be extended until we arrive at the point where the fibrinous matter is equal to the half of the calorifiant, which is probably, in a perfectly normal physiology, the greatest relative proportion of nutritive material admissible.

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[*] Albuminous matters contain about 53 per cent. of carbon, 7 of hydrogen, 16 nitrogen, and 24 oxygen. Hence to obtain the carbon from the above table we have $\frac{\cdot 25 \times 53}{16} = \cdot 828$ lbs. carbon; for the hydrogen $\frac{\cdot 828 \times 7}{53} = \cdot 109$ lbs. hydrogen; fo the oxygen $\frac{\cdot 109 \times 24}{7} = 0\cdot 373$ lbs. oxygen.

The proportion of the nutritive to the calorifiant constituents of food should therefore vary according as the animal is in a state of exercise or rest; and it is upon the proper consideration of such relations that the true laws of dieting depend. For calculations of this nature, tables exhibiting the amount of albuminous matters in the different articles of food are indispensable, as they afford at a glance the required knowledge. The constituents of the flours used in human food are principally albuminous matter, calorifiant matter, water, and salts; so that, when we have determined the amount of albuminous substance in the dried condition of the flour, the remainder may be estimated as calorifiant matter without any sensible error. In the following table the water has not been removed from the flour. The numbers are the results of my experiments:—

	Albuminous or Nutritive Matter per cent.
Bean meal	25·36
Linseed meal	23·62
Scotch oat meal	15·61
Semolina	12·81
Canadian flour	11·62
Barley	11·31
Maize	10·93
Essex flour	10·55 to 11·80
East Lothian flour	9·74 to 11·55
Hay	9·71
Malt	8·71
Rice (East Indian)	8·37
Sago	3·33
South Sea arrow root	3·21
Tapioca	3·13
Potatoes	2·23
Starch (wheat)	2·18
Swedish turnips	1·32

The numbers represent the amount of albuminous matter contained in 100 parts of the various substances as they occur in commerce. As all of the substances in the table contain from 5 to 14 per cent. of water, certain deductions are required, to arrive at the true amount of calorifiant matter. In general, it may be stated that wheat flour, maize, barley, and beans contain from 10 to 14 per cent. of water, while oatmeal contains 6 per cent., and tapioca, arrow-root, and sago, from 10 to 13 per cent. In order to arrive at the true amount of calorifiant matter contained in the substances in the table, we have only to deduct the amount of albuminous substances, with the water and salts, which, upon an average, amount together to about 12 to 15 per cent. Then, by dividing the remainder, or calorifiant matter by the amount of albuminous substances, we obtain the relation subsisting between the nutritive and calorifiant constituents. In this manner tables may be constructed, illustrating the true practice of dieting.

Approximate Relation of Nutritive to Calorifiant Matter.

	Relation of Nutritive to Calorifiant.
Milk.—Food for a growing animal	1 to 2
Beans	1 — 2¼
Oatmeal	1 — 5
Semolina Barley	1 — 7
English Wheat Flour.—Food for an animal at rest	1 — 8
Potatoes	1 — 9
Rice	1 — 10
Turnips	1 — 11
Arrow-root Tapioca Sago	1 — 26
Starch	1 — 40

From this table we are led to infer that the food destined for the animal in a state of exercise should range between milk and wheat flour, varying in its degree of dilution with calorifiant matter according to the nature and extent of the demands upon the system. The animal system is thus viewed as in an analogous condition to a field from which different crops extract different amounts of matter from the soil, which must be ascertained by experiment. An animal at rest consumes more clorifiant food in relation to the nutritive constituents than an animal in full exercise. The food, therefore, employed by a person of sedentary habits should contain more calorifiant and less nutritive matter than one whose occupations cause him to take more exercise. It is to be desired that some light should be thrown on this subject by careful experiments. The food of animals and the manure of plants we thus, see, afford somewhat of a parallelism. Milk may therefore be used with a certain amount of farinaceous matter, such as the class of flours and meals, with probable advantage; but the dilution should not exceed the prescribed limits. It is thus that we may explain the fact of beans, oats, oat meal, and barley meal being used so extensively in the feeding of horses. These articles of food, however, do not suffice alone; calorifiant matter in the form of hay should be administered. From this table, likewise, we infer that, as nature has provided milk for the support of the infant mammalia, the constitution of their food should always be formed after this type. Hence we learn that milk, in some form or other, is the true food of children, and that the use of arrow-root, or any of the members of the starch class, where the relation of the nutritive to calorifiant matter is as

1 to 26 instead of being as 1 to 2, by an animal placed in the circumstances of a human infant, is opposed to the principles unfolded by the preceding table. In making this statement, I find that there are certain misapprehensions into which medical men are apt to be led at the first view of the subject. To render it clearer, let us recall to mind what the arrow-root class of diet consists of. Arrow-root and tapioca are prepared by washing the roots of certain plants until all the matter soluble in water is removed. Now, as albumen is soluble in water, this form of nutritive matter must in a great measure be washed away: under this aspect we might view the original root before it was subjected to the washing process, to approximate in composition to that of flour. If the latter substance were washed by repeated additions of water the nitrogenous or nutritive ingredients would be separated from the starchy or calorifiant elements, being partly soluble in water, and partly mechanically removed. Arrow-root, therefore, may be considered as flour deprived as much as possible of its nutritive matter. When we administer arrow-root to a child it is equivalent to washing all the nutritive matter out of bread, flour, or oatmeal, and supplying it with the starch; or it is the same thing aproximately as if we gave it starch; and this is in fact what is done, when children are fed upon what is sold in the shops under the title of farinaceous food, empirical preparations of which no one can understand the composition without analysis. Of the bad effects produced in children by the use of these most exceptionable mixtures, I have had ample opportunities of forming an opinion, and I am inclined to infer that many of the irregularities of the bowels, the production of wind, &c., in children, are often attributable to the use of such unnatural species of food. How often are the ears of parents and nurses distressed with the agonizing cries of the helpless child, and how often are these symptoms of suffering treated as the effects of ill-humour, or of causeless peevishness; when, on the contrary, they have been produced by the improper diet in many cases with which the child has been supplied? It should be remembered that all starchy food deprived of nutritive matter is of artificial production, and scarcely, if ever, exists in nature in an isolated form. The administration of the arrow-root class is therefore only admissible when a sufficient amount of nutritive matter has been previously introduced into the digestive organs, or when it is inadvisible to supply nutrition to the system, as in cases of inflammatory action. In such instances the animal heat must be kept up, and for this purpose calorifiant food alone is necessary. This treatment is equivalent to removing blood from the system, since the waste of the fibrinous tissues goes on, while an adequate reparation is not sustained by the introduction of nutritive food. A certain amount of muscular sustentation is still, however effected by the use of arrow-root diet; since, according to the preceding tables, it contains about one third as much nutritive matter as some of the wheat flours. The extensive use of oatmeal, which is attended with such wholesome consequences among the children of all ranks in Scotland, is, however, an important fact deserving of serious consideration; and, it appears to me, is strongly corroborative of the principles which I have endeavoured to lay down in the preceding pages. After the explanations which have been given, it is scarcely necessary to particularize further the specific nature of the food to be recommended for the use of children. A certain admixture of milk, the natural type of the food, is still to be retained, while the solid matter to be prepared along with it may be of great variety, such as bread made into panado, semolina, or pounded wheat; I believe this kind of food, which is sold in the shops, to be generally prepared from wheat brought from a more temperate region than that of

this country, in consequence of the amount of nitrogen which I have found in it. The best American wheat flour, good Scottish oatmeal, and barley meal, may all be employed at different times by way of variety, and repeated according to their agreement with the child's organs of digestion. The digestion of all these forms of food containing starch is greatly promoted by long boiling either with water or milk, as this process is just so much labour saved to the intestinal organs. It is thus obvious that we have a great variety of food fitted for children of which we know the composition, and that we should prefer it to any species of compound stuff the composition of which we are ignorant. It is a sufficiently remarkable fact, that oats increase in nutritive power in proportion to the increase of latitude within certain limits, while wheat follows an inverse law.

In the table which contains the amount of albuminous matter in different kinds of food, a second column, in accordance with tables of this description, might have been added, representing 100 parts of beans as equal in nutritive power to 1160 of starch; but if the views now explained are legitimate, we see that such a method of estimating nutritive power is not founded on scientific principles. In a correct plan of dieting, the proper equilibrium must be retained between the demands of the animal organism and the constitution of the food, otherwise, either the nutritive or calorifiant system must be deteriorated. These views sufficiently explain the experiments which have been made upon cows; in which the result was unfavourable, when they were fed on potatoes and beetroot in considerable quantities, as both of these substances contain an excess of calorifiant matter. It is well known to feeders of cattle, that an animal fed on large quantities of potatoes is liable to complaints, such as affections of the skin, and also to loss of weight. These consequences, it may be readily inferred, are derived from the want of the proper balance between the elements of the food.

The importance of attention to the proper equilibrium of the constituents of the food is clearly pointed out in the following table, from which it is evident, that food containing the greatest amount of starch or sugar does not produce the largest quantity of butter, although these substances are supposed to supply the butter; but the best product of milk and butter is yielded by those species of food which seem to restore the equilibrium of the animals most efficiently. The first column in the table represents the food used by two cows; the second column gives the mean milk of the two animals for five days; the third, the butter during periods of five days; while the fourth contains the amount of nitrogen in the food taken by both animals during the same periods:—

		Milk in five days.	Butter in five days.	Nitrogen in food in five days.
I.	Grass . .	114 lbs.	3·50 lbs.	2.32 lbs.
II.	Barley and hay .	107	3·43	3·89
III.	Malt and hay .	102	3·20	3·34
IV.	Barley, molasses, and hay	106	3·44	3·82
V.	Barley, linseed, and hay	108	3·48	4·14
VI.	Beans and hay .	108	3·72	5·27

We may infer, from these results, that grass affords the best products, because the nutritive and calorifiant constituents are combined in this form of food, in the most advantageous relations. The other kinds of food have been subjected to certain artificial conditions, by which their equilibrium may have been disturbed. In the process of hay-making, for example, the colouring matter of the grass is either removed or altered; a portion of the sugar is washed out or destroyed by fermentation, while certain of the soluble salts are removed by every shower of rain which falls during the curing of the hay. Perhaps similar observations are more or less applicable to the other species of food enumerated.

Bread, on the Preparation of. (From the same.)—The principles which we have been endeavouring to explain being understood, little difficulty will be experienced in constructing dietaries, so as to meet the wants of the animal system under the particular circumstances in which it may be placed. By various mixtures of one kind of flour, less supplied with azotized matter, with another which is richer in this material, the equilibrium of the food which from meteorological causes prevailing in any particular country, may not have reached the proper standard, may be effectually restored. The wheat of England, for example, is inferior to that of the continent of Europe, and of America, as appears from the table. It may, however, be improved by an admixture either with foreign flour, or with oatmeal, barley, beans, or any of those substances which stand above it in the table; and in this state it will be found to form palatable bread. All these species of grain owe their nutritive properties to the presence of fibrin, casein, glutin, and albumen. It is in the predominance of glutin over the other azotized materials that wheat owes its superior power of detaining the carbonic acid engendered by fermentation, and thus communicating to it the vesicular spongy structure so characteristic of good bread. By mixing one-third of Canada flour with two-thirds of maize, a very good loaf is produced, and when equal parts of flour and oatmeal, or of barley, or of peasmeal, are employed, palatable bread is the result. Beneficial effects would probably follow from the admixture of two or three different kinds of grain, and many of these forms of bread might be substituted with advantage for pure wheat flour in peculiar conditions of the system.

When it is proposed to make a loaf of oatmeal and flour, the common oatmeal should be sifted so as to obtain the finest portion of the meal, or it may be ground to the proper consistence. This should be mixed then with an equal weight of best flour, Canadian, for example, and fermented. I have not succeeded in making a good loaf with a smaller amount of flour than one-half, although I have tried it in various proportions. If we were to attempt to raise oatmeal without an admixture of flour, in consequence of the absence of glutin, that principle which retains the carbonic acid of fermentation, we should obtain only a sad, heavy, doughy piece of moist flour. This form of bread, it appears to me, and to many who have examined it, would be a great improvement on the hard, dry oat cakes, so much used in the more unfrequented parts of our country, where the inhabitants have scarcely as yet commenced to share in what are in other localities considered to be necessaries of life. It is an observation which all must have made who have considered the condition of mankind in their various stages of advancement, that an increase in the physical comforts, and above all, the improvement in the diet, are the first symptoms of an onward movement in civilization. It has always appeared to me, that it is in vain to expect any other condition than that of retrogression among people, such as are too abundant in Scotland, and Ireland, where the clothing is so deficient as to leave the extremities of the body, more particularly among the female classes, the educators of the community, in a state of nudity, and where the food is confined in a great measure to the watery potato, or the dry and unpalatable oat-cake.

Maize bread may be made of good quality by a smaller admixture of flour than is necessary in the instance of oatmeal. For this purpose, it should be reduced to a fine meal,—finer than is usual in America. It may then be mixed with one-third its weight of best flour, and fermented in the usual way. When thus prepared, the best maize bread is always dark-coloured, and cannot be made much lighter than coarse wheat bread. The shade, however, is somewhat different from that of wheat, as it inclines more to a yellow tint. We may be quite certain, however, when we see what is called maize bread possessed of a white colour, that it contains much more than one-third its weight of wheat flour mixed with it. Even when one-half its weight of wheat flour is added to it, the dark colour, characteristic of maize, is retained. In these cases, of course the price of the bread must be higher than when a smaller amount of maize is present.

The whitest bread, however, is made by an intermixture of barley meal and wheat flour. The smallest amount of wheat flour in this mixture, which I have found requisite to make a good loaf, was one-half, although the quantity of flour may be diminished according to the increase in the richness of wheat in albuminous matter; an observation which, of course, applies to the various kinds of bread to which allusion has already been made. The most successful of these varieties of bread is, perhaps, that which is made with equal quantities of pease-meal and flour, so far as respects the exterior aspect. The last, however, is palatable, and the specimen is a good example of a wholesome, condensed vegetable diet, and would probably answer as a substitute for animal food where the functions of the stomach are not materially impaired.

Upon similar principles, excellent biscuits may be made, either for rapid consumption, or for preservation, at a more moderate expense than when they are entirely composed of wheat flour. When a biscuit is formed of Indian corn, without any intermixture of wheat, the colour has a yellow tint, which, however, in a great measure disappears when wheat flour is added in the proportion of one-third. When destitute of the presence of wheat, it is not so consistent, and is apt to crack and break off short. Oatmeal and barley-meal biscuits may be produced also by mixture with wheat flour. They require, however, a somewhat larger proportion of the latter, as their particles seem even less adapted of themselves to cohere than those of the Indian corn. An admixture of a variety of meals forms a very palatable biscuit, as it possesses a sweeter taste, even without the artificial addition of sugar, than wheat flour alone. Such biscuits are calculated to keep for a longer or shorter time, according to the firing to which they are subjected. In the former case they are well calculated to keep at sea.

Bread of such a description may be made either by the usual process of fermentation, or by the action of hydrochloric acid upon sesquicarbonate of soda. In many respects the latter process deserves the preference, when we consider the chemical nature of the two methods.

The vulgar idea, which yields the palm of superiority to the former, does not appear to be based on solid data, and it seems desirable, that in a case of so much importance in domestic economy, the arguments in favour of such an opinion should be subjected to a careful experimental examination. Judging, *à priori*, it does not seem evident that flour should become more wholesome by the destruction of one of its important elements, or that the vesicular condition engendered by the evolution of carbonic acid from that source, should at once convert dough (if it were unwholesome,) into wholesome bread.

When a piece of dough is taken in the hand, being adhesive, and closely pressed together, it feels heavy, and if swallowed in the raw condition, it would prove indigestible to the majority of individuals. This would occur from its compact nature, and from the absence of that disintegration of its particles which is the primary step in digestion. But, if the same dough were subjected to the elevated heat of a baker's oven, 450°, its relation to the digestive powers of the stomach would be changed, because the water to which it owed its tenacity would be expelled, and the only obstacle to its complete division and consequent subserviency to the solvent powers of the animal system would be removed. This view of the case is fully borne out by a reference to the form in which the flour of the various species of *cerealia* is employed as an article of food by different nations. By the peasantry of Scotland, barley-bread, oat-cakes, peas bread, or a mixture of peas and barley-bread, and also potatoe-bread, mixed with flour, are all very generally employed in an unfermented form with an effect the reverse of injurious to health. With such an experience, under our daily observation, it seems almost unnecessary to remark, that the Jew does not labour under indigestion when he has substituted, during his passover, unleavened cakes, for his usual fermented bread; that biscuits are even employed when fermented bread is not considered sufficiently digestible for the sick; and that the inhabitants of the northern parts of India and of Affghanistan very generally make use of unfermented cakes, similar to what are called *scones* in Scotland. Such, then, being sufficient evidence in favour of the wholesomeness of unfermented bread, it becomes important to discover in what respect it differs from fermented bread. Bread making being a chemical process, it is from chemistry alone that we can expect a solution of this question. In the production of fermented bread a certain quantity of flour, water, and yeast, are mixed together, and formed into a dough or paste, and are allowed to ferment for a certain time at the expense of the sugar of the flour. The mass is then exposed in an oven to an elevated temperature, which puts a period to the fermentation, expands the carbonic acid, resulting from the decomposed sugar and air contained in the bread, and expels the alcohol formed, and all the water capable of being removed by the heat employed. The result gained by this process may be considered to be merely the expansion of the particles of which the loaf is composed, so as to render the mass more readily divisible by the preparatory organs of digestion. But as this object is gained at a sacrifice of the integrity of the flour, it becomes a matter of interest to ascertain the amount of loss sustained in the process. To determine this point, I had comparative experiments made upon a large scale with fermented and unfermented bread. The latter was raised by means of carbonic acid generated by chemical means in the dough. But to understand the circumstances, some preliminary explanation is necessary. Mr Henry of Manchester, in the end of last century, suggested the idea of mixing dough with carbonate of soda and muriatic acid, so as to disengage carbonic acid in imitation of the usual effect of fermentation; but with this advantage, that the integrity of the flour was preserved, and that the elements of the common salt required as a seasoner of the bread was thus introduced, and the salt formed in the dough.

The result of my experiments upon the bread produced by the action of hydrochloric acid upon carbonate of soda, has been, that in a sack of flour there was a difference in favour of the unfermented bread to the amount of 30lbs. 13oz., or, in round numbers, a sack of flour would produce 107 loaves of unfermented bread, and only 100 loaves of fermented bread of the same weight. Hence it appears, that in the sack of flour by the common process of baking, 7 loaves, or 6½ per cent of the flour are driven into the air and lost. An important question now arises from the consideration of the result of this experiment: Does the loss arise entirely from the decomposition of sugar, or is any other element of the flour attacked?

It appears from a mean of eight analyses of wheat flour from different parts of Europe by Vauquelin, that the quantity of sugar contained in flour amounts to 5·61 per cent. But it is obvious that, as the quantity lost by baking exceeded this amount by nearly one per cent., the loss cannot be accounted for by the removal merely of the ready-formed sugar of the flour. We must either ascribe this extra loss to the conversion of a portion of the gum of the flour into sugar and its decomposition by means of the ferment, which is highly probable, or we must attribute it to the action of the yeast upon another element of the flour; and if we admit that yeast is generated during the panary fermentation, then the conclusion would be inevitable, that another element of the flour, beside the sugar, or gum, has been affected. For Liebig has well illustrated the fact that when yeast is added to wort, ferment is formed from the gluten contained in it, at the same time that the sugar is decomposed into alcohol and carbonic acid. Now, in the panary fermentation, which is precisely similar to the fermentation of wort, we might naturally expect that the gluten of the flour would be attacked to reproduce yeast.

A wholesome and palatable bread may be produced by the employment of ammoniacal alum, and carbonate of ammonia, or soda as a substitute for yeast. In this process the alum is destroyed by the heat: the bread is vesicular and white, and rises, according to the judgment of the baker, as well as fermented bread. It is obvious that none of the ingredients added can affect the integrity of the constituents of the flour; an occurrence which may possibly happen in the preparation of bread by the common process of fermentation, as has been shown even to the azotized principles of the flour. The disadvantages of such a deterioration is sufficiently evident, if we view these principles as the source of nutrition in flour.

A good method of making unfermented bread is to take of flour 4 pounds. Sesquicarbonate of soda, (supercarbonate of the shops) 320 grains. Hydrochloric acid, (spirit of salt or muriatic acid of the shops) 6½ fluid drachms. Common salt, 300 grains. Water, 35 ounces by measure. The soda is first mixed with the flour very intimately. The salt is dissolved in the water, and added to the acid. The whole being then rapidly mixed as in common baking. The bread may either be baked in tins or formed like cottage loaves, and should be kept from one to two hours in the oven. Should the bread prove yellow, it is a proof that the soda has been in excess, and indicates the propriety of adding a small additional portion of acid; the acid varyings omewhat in strength. The same process may be employed in raising the other mixture previously recommended.

PATHOLOGY.

Case of Partial Aneurism of the left Ventricle of the Heart. By JONATHAN PEREIRA, M. D. &c.—Harriet Coventry, aged 15, a servant of all work, was admitted into the London Hospital, under the care of Mr Luke, on the evening of Saturday, February 15th, 1845, with a wound in the lower part of the neck.

It appeared that on Friday, February 7th, she had been sent by her mistress for some beer, and that in walking, with the jug in her hand, she stumbled, and in her fall broke the jug, a fragment of which cut her neck. Some dark-coloured blood escaped, and she became faint. She was taken home and kept quiet, and the wound was supposed to be going on favourably till Thursday, April 13th, when, after some exertion, hæmorrhage recurred during the night.

February 15th.—On her admission into the hospital a wound about an inch in length was discovered in the neck, a little above the sternum, and from which venous blood was oozing. No attempt at reparation had apparently been made, as there was neither adhesion of the edges of the wound, nor purulent secretion. The wound was dressed, and the patient put to bed.

February 16th.—On dressing the wound this morning, a small quantity of venous blood escaped. The attention of the dresser being accidentally directed to the condition of the heart and great vessels, a loud auomalous murmur under the sternum was discovered; and in consequence, Mr Luke's attention was drawn to the case.

February 17th.—To-day I saw the patient for the first time; and gathered from her the following history of her case. About three years ago, she had suffered a severe attack of rheumatism with fever, which confined her for several weeks to the bed. Up to that time she had always enjoyed good health; but subsequently had not been able to run up stairs or walk quickly without suffering unpleasant sensations, which induced her to stop. On questioning her whether she had ever experienced palpitation and shortness of breath, she replied yes. She had neither fainted, nor suffered with swollen feet; and altogether her symptoms had not been sufficiently severe to induce her to seek medical advice.*

At this time (February 17th), she appeared in excellent health and spirits. She was plump and had florid cheeks; her body was well nourished, and properly supplied with arterial blood. She laughed and seemed much amused at the interest manifested by her medical attendants in the condition of her chest. She told me that her mother said there had always been a remarkable noise at her chest; but though I took some pains on this point, I could not elicit any satisfactory explanation of the noise referred to.† The catamenia had not appeared. The resonance of the chest from percussion was normal. The dulness of the præcordial region was not greater than natural, whether considered in reference to its extent or intensity.

The impulse of the heart was perhaps rather above that of health; but not remarkably so.

But the most remarkable sign was, a very loud systolic, apparently superficial murmur, under the sternum, and on both sides of it. In character it was between the bellows and saw sounds. The extent of space over which it was audible was considerable; and it was not very easy to determine the locality of its maximum intensity. Indeed, at two spots, about one inch and a half asunder, the sound appeared of nearly equal intensity; and I, therefore, inferred that it consisted of at least two simultaneous murmurs. It was very loud under the sternum towards the left side, and opposite to the space between the cartilages of the third and fourth ribs;[*] or, as was presumed, over the semilunar valves. It was also very loud under the sternum, rather to its right edge and opposite to the cartilage of the fifth rib. Moreover, between these spots the sound was also strong; but scarcely so intense as at the points just mentioned.

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* After the death of the patient, I obtained from her mother some particulars respecting the previous history of the daughter, which agree in the main with those furnished me by the patient herself. The mother tells me that so good had been her daughter's health, previous to the rheumatic attack, that, to use her own language, a single sixpence had never been expended on the girl for medical advice. During the rheumatism the girl's chest became so much affected, that her mother judged it expedient to call in a medical man (whose name she had forgotten), who listened to her chest and ordered a mustard poultice to be applied to it. The breathing at this time was, to use the mother's language, "very heavy." The girl suffered for a considerable period from the effects of this attack; she was reduced almost to a skeleton, and experienced great pain when she attempted to walk. Gradually, however, she recovered, though she never quite regained her former healthy state. From time to time she complained of pain at her chest, and occasionally suffered with shortness of breathing. Running up stairs greatly fatigued her. At times her legs swelled very much. This latter statement was in opposition to the information furnished me by the girl herself; but the mother persists in its accuracy. No blueness of the lips had been observed. Both father and mother were subject to rheumatism. The mother had been laid up with it for ten weeks, just before the daughter's attack.

† When I subsequently questioned the mother respecting this noise, she replied, that her daughter had experienced a kind of rattling noise at her chest, when she had a cold, especially when she was sleeping!

After listening very attentively for some time, I concluded that the maximum intensity was at the first spot named, that is, over the semilunar valves. Mr Luke, who was with me when I made the examination, was at first doubtful as to the locality of the greatest intensity of the sound; but on re-examination he coincided in opinion with me.[†]

The murmur was very loud over the whole of the right side of the heart. On the right side of the chest, between the cartilages of the third and fourth ribs, near to the sternum, or, as was presumed, over the right auricle, it was very audible. Higher up the chest it was less loud. It was very distinct over the *arteria innominata*, and was perceptible in the carotid arteries. On the left side of the chest there was not any distinct murmur above the second rib. Posteriorly, the murmur was heard, though feebly, on the third dorsal vertebra, and also somewhat below this.

Another very marked symptom was a strong purring tremor, most perceptible on the right side, in the space between the cartilages of the third and fourth ribs near to the sternum, or, as was presumed, over the right auricle.

Especial attention was paid to the condition of the venous system. But no evidence of venous congestion was observed. There was neither a purple or livid tint of countenance, nor fulness or pulsation of the jugular veins: on the contrary, the patient's colour was florid, and that of a person well supplied with properly arterialized blood.

The pulse was slightly quickened, regular, soft, and rather full, and not jerking. No difference could be detected between the pulse of the two wrists. The tongue was moist and clean.

February 19th.—Since my last visit hæmorrhage from the wound had occurred twice; once to the extent of a few drops only, but this morning to the amount of several ounces. The girl is suffering with a slight attack of fever; her pulse is somewhat quickened, her tongue furred; and she appears weaker than when I saw her last. The murmur remains the same; but the purring tremor is scarcely so strong.

February 20th.—Febrile symptoms augmented; face flushed, but of a bright vermilion hue; tongue coated; skin hot; pulse 98, soft, rather full.

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[*] The above statements as to the seat of the sounds have reference to the horizontal position of the patient, who was examined as she lay on her back in bed.

[†] Subsequently to the death of the patient, I have been favoured with notes of the case taken by Mr Cumming, one of the house-surgeons, as well those taken by Mr Tripe, a former pupil of the hospital, now in practice in the Commercial Road. Their notes of the seat of the greatest intensity of sound neither agree with each other nor with mine. Mr Cumming states that he thought that the murmur was loudest "just at the right border of the sternum, at its centre;" while Mr Tripe considered the murmur to be loudest under the sternum, "opposite the junction of the fifth rib, and to the right side." It is but proper, however, to observe that our notes of the case were not made on the same day. Mine were made on the 17th, when the girl was in apparent health; while Mr Tripe's were made on the 20th, that is, after symptoms of fever had come on. Those of Mr Cumming are not dated.

She had vomited repeatedly, was very thirsty, and had lost her appetite. Effervescing salines and a purgative were administered, but without much relief. In the evening a slight attack of shivering occurred.

February 21st to the 28th.—The symptoms continued much of the same kind for several days, but the girl was obviously getting weaker. On the 24th the vomiting ceased. On the 26th she complained of pain in the epigastrium. On the 27th she lay in a half-sleepy or soporose condition, moaning continually. On the 28th respiration was hurried; pulse 130. She lay with her eyes closed, without taking the least notice of surrounding objects. A slight purulent discharge was observed coming from the right eye, and the cornea was somewhat opalescent.

March 1st.—Complete insensibility; pulse 145.

She died on March 2d.

The treatment consisted in the use of mild purgatives, effervescing salines, hydrocyanic acid to check the sickness, calomel given so as to affect the mouth slightly; leeches to the epigastrium, and afterwards to the eyelids.

Post-mortem appearances.—I was unable to be present at the examination of the body, and, therefore, with the exception of the description of the heart, which I subsequently drew up, the following notes of the appearances were made by others, principally by Mr Cumming.

The wound in the neck was about half an inch above the sternum; its length was one inch, its breadth about three-quarters of an inch. It extended through the anterior layer of the deep cervical fascia, to a sinus communicating between the anterior jugular and thyroid veins. From this sinus (into which the wound had penetrated), a large vein passed directly into the left *vena innominata.* Two or three drachms of pus were observed in the wound.

The brain was somewhat congested, and presented a large number of bleeding points. The lungs were healthy. Nothing remarkable was observed in the liver. The spleen was much congested and very soft. Several purulent deposits were observed in it; the tissue around being much congested. In both kidneys, numerous abscesses, varying in size from a pin's head or a pea, were discovered. Within the sac of the peritoneum there were about two ounces of a turbid fluid, containing a small quantity of pus. The cornea of the right eye was opalescent; and small portions of pus were observed floating in the aqueous humour of the anterior and posterior chambers.

The *venæ cavæ, innominatæ,* and *iliacæ*, did not present any marks of inflammation.

Heart.—The pericardium presented evidences of having formerly suffered from inflammation; patches of firm adherent lymph being observed in various places. That portion of the pericardium which invested the right auricle was studded over with numerous small granular deposits.

The muscular tissue of the heart was quite healthy. Especial attention was paid to it with reference to the existence of fatty degeneration; but I was unable to detect the least traces of this change.

The right ventricle and auricle were of the natural size and thickness: neither dilatation nor hypertrophy having taken place. The pulmonary artery and its semilunar valves were healthy: perhaps the *corpora Arantii* were somewhat smaller than natural. The *foramen ovale* was patescent; but the aperture was oblique and valvular, and probably did not give any inconvenience.

The left ventricle and auricle appeared to be of the natural size and thickness; though by some persons the walls of the ventricle were thought to be slightly thicker than natural.

An aneurismal communication existed between the left and right sides of the heart. The mouth of the aneurism was situated immediately beneath the right aortic valve and the angle between this and the posterior one. It was bounded above by the right aortic valve, and by the junction of this and the posterior valve, and below by the wall of the left ventricle. It was large enough to admit the tip of the little finger. It was crescent-shaped; the convexity of the crescent being downwards and towards the posterior valve, the concavity upwards and away from this valve. The aperture was bounded at its lower part by the muscular structure of the septum, covered by a firm membrane, on which were some old deposits of lymph which rendered it rough. The sac or cavity of the aneurism was about the size of a small hazel-nut; it was lined by a thin smooth membrane continuous with the internal membrane of the ventricle. At the bottom of it were four pouches (one of which had burst) protruding into the right cavities of the heart. One of these pouches projected into that portion of the right ventricle from which the pulmonary artery springs, and to which some anatomists apply the name of infundibulum or infundibuliform cavity. The pouch was connected with the anterior or larger fold of the tricuspid valve, being adherent to the surface of the fold which faces the pulmonary artery. A second pouch formed a very slight bulging into the right auriculo-ventricular opening at the tendinous margin or ring. The third pouch was situated a little below the second one, and involved the base of the anterior or larger fold of the tricuspid valve. The fourth pouch, that which had burst, had made its way through this fold, and will be described presently.

Of the three divisions or folds of the tricuspid valve, two (viz. the right or posterior fold or "curtain" and the left or internal fold or "fixed curtain") were quite healthy. The anterior and larger fold or "curtain," that which prevents the escape of blood into the pulmonary artery during the diastole of the ventricle, was, however, in a morbid state. Nearly the whole of its auricular surface was covered with firm lymph, and a portion of the fold, about the size of a sixpence, was pouched or dilated, and protruded like a truncated cone. In the centre of the pouch or truncated portion was the aneurismal aperture, the diameter of which was about equal to that of a large quill. Part of the fold (viz. the portion immediately around the pouch, as well as a portion of the free or floating margin) was adherent to the wall of the ventricle, so that a part only of the fold could act as a valve during the systole of the ventricle.

If I have rendered intelligible the position of the terminal aperture of the aneurism, it will be obvious that, during the diastole of the ventricles, when, be it remembered, no blood would be passing through the aneurismal sac from the left ventricle to the right side of the heart, the aperture would be in the right ventricle; and, it is also clear that, had the valve contracted no adhesion to the wall of the right ventricle, the aperture of the aneurism would, during the systole of the heart (at which period blood would be passing through the aneurismal sac from the left to the right side of the heart) have been in the floor of the right auricle,—that is, in the auricular surface of the valve, which, at that period of the heart's action, would constitute a portion of the floor of the auricle. But, owing to partial adhesion, this fold of the valve could not be entirely or completely raised during the ventricular contraction. It appeared, however, to myself and others who carefully examined the preparation in its recent state, that the blood which would escape from the left ventricle of the heart, through the aneurism, would be projected into the right auricle, owing partly to the direction of the terminal aperture during the systole of the heart, partly to the projection from the surface of the valve of the pouch in which the aperture was placed, and partly to the reflux current of blood, which, owing to the imperfect closure of the auriculo-ventricular orifice, would sweep over the aneurismal aperture in passing from the right ventricle to the right auricle at this period.

The aortic valves were not healthy. On all three of them were slight deposits of lymph. The right valve was larger than the others, and, towards its upper or free edge, was partially cribriform or reticulated, having eight or nine small meshes in it. The pocket of the valve hung down and partially covered the mouth of the aneurismal sac. The *corpus Arantii* of this valve was larger than natural.

On the upper part of the ventricular or aortic face of the right (or superior and anterior) lamina of the mitral valve was a large oval patch of firm lymph. Its position was such, that during the diastole of the heart, when this lamina would be guarding the mouth of the aorta, the patch would be directly opposed to the mouth of the aneurismal sac. The size of the patch corresponded to that of the aneurismal mouth.

Small patches of lymph were observed at the commencement of the aorta.

REMARKS.—1. The cause of death was undoubtedly purulent infection of the blood. The symptoms during life, and the appearances after death proved this. Phlebitis is not stated to have been detected on the *post-mortem* examination, nor is it necessary to assume the existence of this disease to account for the presence of pus in the blood; inasmuch as this secretion may have been derived from the wound, and have entered the circulation by the cut orifice in the vein, into which it was probably drawn during inspiration.

2. That the communication between the two sides of the heart was not congenital, but the result of disease, I entertain no doubt. In the *first* place, it appears from the reports of both the patient and her mother, that up to the period of the rheumatic attack, the girl was in perfect health. *Secondly*, it is tolerably clear, that, during this attack, she suffered with both endocarditis and pericarditis. This is inferred from the symptoms referred to the chest, from the mode of examination and local treatment adopted by the medical attendant, and more particularly from the *post mortem* appearances. In the *third* place, this attack seemed to have produced some permanent cardiac lesion, as the patient never completely recovered from it, but subsequently suffered from time to time with pain at the chest, palpitation, short breathing, and swollen feet. *Fourthly*, dissection showed the aneurismal nature of the disease. A pouched sac, lined by a membrane continuous with the endocardium, was found in the interventricular septum; the mouth of the sac being in the left side of the heart, while the pouches projected into the right side, showing that the force by which the sac was distended was derived from the left side; one of the pouches was ruptured, the parts surrounding the orifice being covered with lymph.

I should scarcely have considered it necessary to have recapitulated these facts but for a remark made by Dr Thurnam* in reference to a preparation in the museum of the Royal College of Surgeons, that he had discarded the idea which he had "at first entertained, that the opening in the septum of the ventricles, in this case, was not a congenital malformation." Now, as that case closely agrees in many respects with the one just related, I am anxious that no doubt should be hereafter raised as to the precise nature of the present case.

3. The seat of the aneurism was that part of the septum which is not supplied with muscular fibres. "It is well known to anatomists," says Dr Thurnam,† "that the highest part of the septum, which occupies the angle between the posterior and right aortic valves, and which, in some

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* On Aneurisms of the Heart in the 21st vol. of the Medico-Chirurgical Transactions, p. 254, 255. 1838.

† Ibid. p. 222.

instances of congenital malformation, is deficient, is in the human subject formed not of muscular fibres, but simply of the endocardium of the right and left ventricles almost in apposition, and strengthened only by the interposition of a little fibrous tissue continuous with that of the aorta. In many ruminant animals this point is well secured by an osseous plate; but in man, as a comparatively weak spot, it is perhaps probable that occasionally it may become the seat of aneurismal dilatation. I am not, however, in possession of any fact which proves this to have occurred, unless a case, the preparation of which is in the museum of the Royal College of Surgeons, and which I shall describe when speaking of aneurismal dilatation of the valves of the heart, is to be considered of this nature."

The present case proves, in a very remarkable manner, the correctness of Dr Thurnam's opinion; for the situation of the aneurism is precisely that which he thought might occasionally be the seat of the disease.

4. I have denominated the disease *partial aneurism of the left ventricle bursting into the right side of the heart.* I have preferred this name to that of *spontaneous varicose aneurism of the heart,* proposed for these cases by Dr Thurnam. A *varix* is a vein whose calibre is enlarged, and whose contents are venous blood; and the term *varicose*, it appears to me, is only applicable when a vein, or, to speak more generally, some portion of the venous system, is dilated outwardly. In the present case, there was no external or outward dilatation of the right cavities, and, therefore, I apprehend in strictness the aneurism should not be called a varicose one. The aneurismal pouches which projected into the right cavities had no analogy with varices: they encroached on, not augmented, the diameter of the venous canal, and they contained arterial, not venous, blood.

Careful examination showed that the membrane lining the aneurismal sac was continuous with the endocardium. The aneurism, therefore, was of the *true* species, which, according to Dr Thurnam, constitutes by far the greater proportion of cases of aneurism of the heart.

5. The rupture of an unequivocal aneurism of the left ventricle into the right cavities of the heart, is, if not unique, at least exceedingly rare. In the first place partial aneurism of the heart does not frequently proceed to rupture; for, of fifty-eight cases of aneurism of the left ventricle cited by Dr Thurnam, only nine are stated by him to have burst. In the next place, aneurism of the interventricular septum is somewhat rare; and I have not been able to find notices of more than seven instances of it, and several of these were not originally described as aneurismal. Three of the cases are included by Dr Thurnam in the fifty-eight above alluded to. A fourth is that of the preparation in the Museum of the College of Surgeons to which I have before had occasion to refer, and which Dr Thurnam has described as aneurismal dilatation of the valves of the heart. A fifth has been noticed by Dr Bond*; a sixth is quoted by Bouillaud† from Thibert; and the seventh has been described by Dr Craigie‡.

Of these seven cases three only took place in the base of the septum, namely, the fourth, fifth, and seventh in the above list. But the fourth and fifth are not unequivocal instances of aneurism; and the seventh did not proceed to rupture. So that as a case of unequivocal aneurism of the base of the interventricular septum which proceeded to rupture, the present is, as far as I can learn, an unique case.

6. The right cavities of the heart were neither dilated nor hypertrophied. This is a fact deserving of attention, since it serves to correct Bouillaud's hypothesis that the passage of arterial blood into the right cavities of the heart modifies their nutrition and occasions hypertrophy. "I would not,"

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* Johnson's Medico-Chirurgical Review, 1838.

† Traité Clinique des Maladies du Cœur, 2me éd. t. i. p. 594. 1841.

‡ Edinburgh Medical and Surgical Journal, April 1843.

he observes, "attach too much importance to this theory; but it appears to me well worthy of remark, that in almost all the cases of communication between the right and left cavities, it is the former which become hypertrophied "* In the case of the ventricle this author observes, that the hypertrophy is ordinarily concentric (as if the augmentation of nutrition was principally effected in the fleshy fasciculi on which the exciting cause more directly acted), while in that of the auricle it is always accompanied with dilatation.

7. The absence of all symptoms of venous congestion, such as cyanosis, jugular pulsation, &c. deserves to be especially signalized.

Notwithstanding that, during the systole of the heart, blood was projected by both the ventricles into the right auricle, this cavity was neither dilated nor hypertrophied.

It was suspected during the lifetime of the patient that the right as well as the left side of the heart was diseased, and, therefore, especial attention was paid to the condition of the venous system; but, as I have before stated, no symptoms of congestion of it were discovered. On the contrary, every one noticed the remarkably florid or vermilion tint of the patient.

It may perhaps be said that this was to be expected, inasmuch as the blood which was abnormally conveyed into the right side of the heart was vermilion-coloured. But Dr Hope has observed, that if the arterial blood replace venous blood in the right cavities, a diminished quantity of venous blood will be transmitted to the lungs for arterialization, and thus the total mass in the system will be darkened.

In the present instance, I ascribe the absence of cyanosis and of all other symptoms of venous plethora, to the non-existence of contraction of any of the heart's orifices, and to the healthy condition of the pulmonary artery and lungs, and the consequent facility with which the blood of the right side of the heart traversed the pulmonary vessels.

8. Another point, to which I am anxious to draw attention, is the cribriform condition of a portion of the right aortic valve.

In the 20th volume of the Medico-Chirurgical Transactions, Dr Kingston has described this lesion in an interesting paper, on atrophy of the heart's valves. He ascribes it to "progressive absorption," and regards it as a species of atrophy produced by an undue force of the blood's impulse, combined with debility of the valves' nutritive powers from previous disease. In three out of five cases referred to by Dr Kingston the patients were rheumatic or gouty.

In the present case there was no evidence of any sufficient "undue force of the blood's impulse," to have caused this result. In the opinion of my colleague, Mr Adams, the lecturer on Anatomy in the London Hospital Medical School, there was no hypertrophy of the left ventricle.

Instead of being atrophied, the valve was decidedly larger than natural; and even after the preparation had been immersed in rectified spirit for more than six months, and the parts had consequently become much shrivelled and corrugated, the valve measured somewhat more than seven-eighths of an inch in length.

The thickness of the valve, though variable in different places, was for the most part greater than natural. In one small spot immediately beneath the cribriform portion, however, it was decidedly attenuated and somewhat pouched. Was this the first step in the formation of another perforation or mesh?

9. It has been already remarked that there was a large patch of firm lymph on the right lamina of the mitral valve, in such a position that, during the diastole of the heart, when this lamina would be guarding the

¶ Traité 2nde, p. 694.

aortic orifice, the patch would be directly opposed to the mouth of the aneurismal sac.

Was the position of this patch accidental? or had it relation to the seat of the aneurism? We know that in pleuritis, the surface opposed to the inflamed portion of the pleura frequently becomes inflamed, apparently in a secondary manner. Does the same hold good with regard to inflammation of the endocardium? In other words, was the inflammation of that portion of the endocardium which subsequently became the seat of the aneurism, the cause of the deposit on the opposed surface of the mitral valve?

10. The age of the patient was remarkable. In thirty-five cases of aneurism, cited by Dr Thurnam, there is only one case occurring in a patient under twenty-one years; the youngest patient was eighteen, the oldest, eighty-one years old.

Corvisart (quoted by Bouillaud)*, mentions the case of a boy, aged twelve and a half years, who had, during five months, organic disease of the heart. He died of suffocation, and on examination of the body there was found perforation of the interventricular septum; but there was no aneurismal cyst.

Dr Thurnam has, I think, assigned the true reason of the greater frequency of cardiac than of arterial aneurism in early life; namely, that the former is frequently referable, (as in the present case), to rheumatic endocarditis, a disease of frequent occurrence in young persons.

11. The last topic on which I propose to remark, relates to the murmur and purring tremor.

The murmur was chiefly remarkable for its very great loudness, and for its seat. It was systolic; that is, it occurred at the time of the ventricular contraction and arose chiefly, if not entirely, from the passage of blood into and out of the aneurismal sac. In fact, it may be regarded as a compound murmur, made up of two or three murmurs of simultaneous occurrence; but which were scarcely to be separately distinguished. The loudest sound (murmur) appeared to both myself and Mr Luke to exist at the aortic orifice, and was supposed to have been produced at, or close to the sigmoid valves. This must have been produced by the entrance of the blood into the sac. There was also a very loud murmur over the right auriculo-ventricular orifice, and which was probably produced principally by the escape of blood from the aneurismal sac. But as there must have been regurgitation from the right ventricle into the right auricle, and as the regurgitating blood would pass over the roughened portion of the tricuspid valve, it is probable that a murmur would in this way also be produced, and contribute to the loud sound heard at this spot. The dilatation and distension of the aneurismal sac may also perhaps have yielded a murmur.

There is one point connected with this subject deserving attention, as serving to distinguish the murmur heard in this case from ordinary obstructive murmur of the aortic valves; I refer now to the very loud murmur over the commencement of the aorta, and the very slight or scarcely audible murmur in the carotids. There was no diastolic murmur; at least, after a most careful examination, I could not detect any. Gendrin† states, that in four cases of aneurism of the walls of the heart, he recognised a second or diastolic murmur, which he ascribes to the return of the blood by a narrow orifice, from the cavity of the aneurismal sac into the cavity of the ventricle, when the latter is beginning to relax. But in the present case there was probably little or no blood returned from the sac into the left ventricle, on account of the position of the terminal aperture of the aneurism, and the obliquity of the aneurismal sac. Moreover, during the diastole of the ventricle the mouth of the aneurismal sac would be closed by the right lamina of the mitral valve.

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* Traité, t. i. p. 591.

† Leçons sur les Maladies du Cœur, t. l, p. 109. 1841-2.

In consequence of the cribriform condition of a portion of the right aortic valve, aortic regurgitation might be expected. If this really took place there was not any perceptible murmur produced by it. But from the perforated portion being of small extent, and near the free edge of valve, while the valve itself was unusually large, I am not satisfied that regurgitation from this condition actually occurred, at least to any great extent.

Loud purring tremor over the right auricle was in the present case a very marked symptom. It occurred at the moment of the systole of the ventricles, that is, at the time when the blood was being propelled into the right auricle from both the ventricles. It was probably caused by the interference of the two currents of blood, proceeding from the ventricles, with each other, as well as with the blood returning into the auricle from the vena cava.

Finsbury Square, September 29th 1845.

TOXICOLOGY.

Opium not poisonous to the rabbit. By M. Lafargue. (*Comptes Rendus des Seances de l'Academie des Sciences,* March 1845.) M. Lafargue having observed that the poppy was a favourite food with the rabbit, on which it throve and got fat, felt some curiosity to ascertain whether that animal would be affected by opium. For this purpose he dissolved three grains of the acetate of morphia in a quantity of water, and mixed with a certain quantity of bran, which a rabbit eat in two days, but was not in the least affected by it.

Case of poisoning by Carbonate of Lead. By M. Schubert. (*Journal de Pharmacie et de Chimie,* June 1845.)—A man, 20 years of age, troubled with heartburn, took by mistake, instead of chalk, about 300 grains of the carbonate of lead. Shortly after he was seized with sense of burning in the epigastric region, followed by obstinate vomiting. Twenty-four hours after he had swallowed the dose he was seen by M. Schubert, who found him suffering inexpressible agony; the face was swollen; the eyes brilliant and projecting from their orbits; the tongue dry; and the thirst inextinguishable. The abdomen was swollen, and was the seat of acute pain, which the slightest pressure augmented, but which firm pressure rather relieved. The vomitings had ceased of themselves, but the bowels remained constipated. Sulphate of magnesia with opium was therefore administered, as well as the same salt in an oily emulsion, under which all the symptoms rapidly disappeared.

LEGAL MEDICINE.

Action for damage to Vegetation by Muriatic Acid Gas. (Liverpool, September 1st. Gerard v. Muspratt.)—A trial of some importance in relation to chemistry and botany has recently taken place at the Liverpool Assizes.

An action was brought by Sir J. Gerard against Mr Muspratt, a chemical manufacturer, for damages for a nuisance alleged to have been caused to the plaintiff by certain chemical works belonging to the defendant.

The damage consisted in the alleged destruction of a large quantity of timber trees, to the value of L. 5825, by the muriatic acid gas evolved in the manufacture of carbonate of soda from sea salt. Several witnesses deposed that for the last five or six years, *i. e.* since the erection of the alkali works, the vapour had gradually led to the destruction of that portion of the timber on the estate which was exposed to it. Many thousands of the trees had become thus affected. The scientific evidence only will be of interest to our readers, and of this we subjoin the following abstract. The evidence in favour of the plaintiff was as follows :—

William Reynolds, M. D. was a lecturer on chemistry, and had bestowed a great deal of attention to the study of chemistry. Was generally acquainted with the nature of the articles manufactured by Mr Muspratt. There was undoubtedly a noxious vapour which arose from the manufacture of alkali, but when the apparatus was carefully constructed, the escape of vapour was, to some extent, prevented. The gas emitted was muriatic acid gas, which had a strong affinity to water or damp. On issuing from a chimney such as Mr Muspratt's, it would have, in damp weather, the appearance of a thin white smoke. It would depend in a great measure upon the state of the atmosphere as to whether it would fall at a short distance or be conveyed to a greater distance. In a dry atmosphere it would perhaps float a distance of four or five miles before falling near to the earth. The moisture given out in the manufacture of the material was undoubtedly noxious and injurious to vegetation. It had a strong pungent smell, and made the eyes smart. It likewise affected respiration a little. Its effects upon the leaves of trees would be, when weak, to turn them brown, and when strong, to curl them up and make them crimp. Leaves were certainly essential to the health of trees; without them, they would soon die. He had been over the plantations of Sir John Gerard's estate. There were a vast number of trees injured to a great extent. He could suggest no other cause for the injury they had sustained than the gas emitted from the manufactories of alkali in the neighbourhood.

Cross-examined by Mr Serjeant Murphy.—The effects he had seen were only to be attributed to the noxious gas from the alkali works. It was not possible for him to assign any other cause. If the quantity of gas evolved from the chimney were only as 1 cubic foot to 10,000 cubic feet of air, he did not think it would have any effect upon vegetation at all (?). He could not say to what extent the gas would be diminished in its passage through a flue of 500 feet in length, and a chimney of 300 feet in height, having also to pass through a descending current of water. He did not know whether, after floating through the air for 2½ miles, its strength would be decreased or rendered innocuous; it depended, no doubt, to a great extent, upon the state of the atmosphere at the time. He thought grass was less susceptible of its influence than the leaves of trees, as the former was always growing. He should think it probable that if a strong volume of the gas was to pass over a field of grass its effects might be visible; but, of course, it depended in a great measure upon its strength, and the length of time it remained. Did not think that plants were quite so susceptible as trees, the latter being much higher. The flowers might be affected, and yet the plant not be injured.

By Mr Martin.—Was aware that means had been tried to condense this gas, and prevent its escape altogether, but he believed that they were ineffectual as to the entire prevention. The body of gas was too powerful to be overcome by the water through which it was caused to pass in its passage up the chimney. As far as his experience went he had never known any means effectual in preventing the escape of muriatic gas.

John Lindley was professor of botany in the University College of London, and vice-secretary of the Horticultural Society; had devoted a considerable number of years to the study of botany. Had examined the timber on the estate of Sir John Gerard. Had found a great number of trees dead. Had examined the soil in places, and found it very good land. Had done so in places where there were trees actually dead and dying. Trees suffering from damp exhibited an entirely different appearance. Their leaves and bark were preyed upon by numbers of insects. He had examined several of the trees carefully, but they bore no evidence of having been injured in that manner. The injury at its outset was most apparent at the extremities of the upper branches. He had observed, in many instances, *that that side of a tree which was presented towards the alkali works was more injured than the opposite side* The alkali works of Mr Crossfield, as well as those of Mr Muspratt, had been pointed out to him. He had also particularly noticed six Scotch fir trees, which from their age, must have been at one time very healthy; they were planted on a hill, in the form of a lozenge; the one *immediately fronting Mr Muspratt's works* was quite dead, the two behind it were dying, and the other three, which had in some measure been *protected by them,* were comparatively uninjured. This had struck him as a remarkable case, and he had, therefore, particularly noticed them. The muriatic acid gas emitted from the manufacture of alkali was well known to be injurious to trees and vegetation generally. The trees he saw had undoubtedly been affected by it. He had no doubt the appearances he had witnessed amongst Sir John Gerard's woods were solely to be attributed to the noxious vapours in the atmosphere, and not to any natural causes. Wherever the trees had been *protected* by being *in hollows or otherwise,* they were at present uninjured; but if the alkali works were continued to be carried on, he had no doubt all the trees on the estate would be eventually destroyed.

Cross-examined by Mr Serjeant Murphy.—The stag-headed appearance of some of the trees, such as had been described, might sometimes be caused by defects at the roots. It was a common result in bad soils. Could not say whether apple-trees were very susceptible of injury from the effects of the vapour. Some trees were certainly less susceptible than others. He had noticed that oak and ash trees were affected soonest. If flowers were subjected to the vapour they would doubtlessly be destroyed, although the plant might not be injured. If the vapour that passed over a clover field in bloom was sufficiently strong it would no doubt destroy it. It was not possible to say what strength would be necessary, unless by actual experiment.

By Mr Martin.—He had seen no appearance of bad soil on Sir John Gerard's estates that would cause the stag-headed appearance to trees which had been spoken of.

By the judge.—Had seen no natural causes which could account for the injury done to the bark of many trees he had examined. Had particularly noticed a beech that was somewhat exposed towards Mr Crossfield's chimney. On the *side which faced* it the bark was rendered quite hard, and very difficult to be peeled off, while on the opposite side of the tree there was no apparent injury done. There might have been some very few instances of a tree being diseased, when the others around it were in a comparatively healthy state, but it generally happened that three or four trees near together were all alike affected.

In the defence it was urged that the damage to the trees was not due to muriatic acid gas, but to the dampness of the soil, and the burrowing of rabbits, which destroyed the roots. A horticulturist was called, who thought that the causes of decay were quite natural. Dr Miller and Dr Brett gave evidence in favour of the defendant.

Dr Miller stated that he, with Dr Brett and Sir Robert Kane, of Dublin, had made a series of experiments at the works. Muriatic acid gas is evolved in the process; part of it is condensed, and part escapes into the air. There is what is called a decomposing chamber, the gas from which passes into a tower filled with pieces of coke, and through which, from the top, a stream of water is constantly flowing. The gas is thus absorbed by the water. No gas escapes from the flue at the top of that tower. There are besides six reverberatory furnaces on the old plan. Connected with these was a long horizonal flue, communicating with the main chimney. A stream of water flowed through it, which would condense a great portion of the gas. Tested the air as it passed out of that flue into the main chimney at two different periods in the process. The muriatic acid gas, when in the greatest quantity, was in the proportion of 1 to 1866 parts of atmospheric air: in the second experiment, 1 in 6500 parts. The vapour has the greatest tendency to fall in moist weather. It would pass to a greater distance in dry. Thought the gas would be diluted 50 times in half a mile, and that it would not be appreciable at the distance of two miles.

In cross-examination he stated, that if the gas was sufficiently powerful at the distance of two miles, to make the eyes smart, it was certainly appreciable.

In answer to some further questions by his lordship, the witness said the gas was very pungent, and would be perceptible to the smell in extremely minute quantities. White vapour might arise from steam; that, however, would be dispersed in a distance of 100 or 200 yards.

Dr Brett gave evidence not differing materially from that of Dr Miller.

His lordship summed up, and the jury, after deliberating a considerable time, returned a verdict for the plaintiff.—Damages L.1000.

Remarks.—Dr Christison and Dr Turner, however, clearly proved many years ago that a tenth of a cubic inch diluted with 20,000 times its volume of air, so as to be quite *imperceptible to the nostrils*, shrivelled and killed all the leaves of various plants which were exposed to it for twenty-four hours—a fact of which Dr Reynolds does not appear to have been aware; for he is reported to have said that double the proportion would be harmless to vegetation. This destruction of vegetable life is, perhaps, one of the most delicate tests for the presence of this gas. It is, however, a matter of regret, that so important and useful a manufacture, should be, in spite of all precautions, so destructive a nuisance to health and property.

REPORT of the Committee on Quarantine, Regulations read to Annual General Meeting, held 4th May 1846. George Berry, Esq. *Chairman of the Chamber*, in the Chair.—1. In the opinion of your Committee, the Quarantine Laws are, as regards Britain, altogether unnecessary: all diseases, against the introduction of which they have been established, arise, according to the best medical evidence, from climate and local circumstances, are only propagated by epidemic influence, and have never been proved to have been transmitted from one locality to another, unless similar causes and influences were existing;—besides, owing to the distance of Britain from countries where such epidemics prevail, and especially as regards the Mediterranean, assuming the average passage at 40 days, any latent disease, even supposed contagious, would have ample time to develope itself*; and further, no instance can be shown of the importation of any infectious disease by means of shipping. In the recent lamentable case of the *Eclair*, from the coast of Africa, it has been proved, that what might have been yellow fever there, had become, at Portsmouth, the common typhus of this country. As to the intermediate Ports in the Mediterranean, where similar climate, and similar epidemic causes may be supposed to exist, a difference of opinion may prevail with respect to the transmission of contagious disease, but the system of quarantine established there, and the completeness of the Lazarettos, affording every facility to vessels to discharge and re-load, the regulations in force there can scarcely be called oppressive. Yet even in these Ports, the testimony of Mr Turnbull, Consul at Marseilles, proves them to be of little avail.—Mr Turnbull states, that though the coast of France, in his neighbourhood, was peculiarly liable to contagion, supposing contagion to exist, and though vessels were almost daily arriving from plague countries, there was no instance of any expurgater having taken the plague since 1729, one person in that year, when opening a bale, dropped down dead. This of course could not arise from plague, as it does not prove fatal in this way.—Again, in the evidence before the House of Commons in 1825, it was proved that although the intercourse between Turkey, Egypt, and Persia, was great and unrestricted, yet plague was entirely unknown in the last country.

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* Quarantine, it may be noticed, is taken from 40—this being the period stated by the original framers, within which the disease would be developed.

II. But assuming contagious diseases to be carried by shipping, and these laws necessary, the term of quarantine ought to be proportioned to the length of the voyage. It appears absurd that a vessel which has made the passage, say from Alexandria to Britain in 40 days, and another which has taken 60, should be detained an equal time by quarantine restrictions.

III. Still, assuming such laws to be necessary, proper facilities ought to be afforded to shipping for discharging and re-loading, by means of Lazarretos, as at Malta, Leghorn, and Marseilles. In England, not even at Milford Haven, or the Thames, is there, in the words of M'Culloch, 'a Lazaretto where a ship from a suspected place may discharge her cargo and refit,—she is detained frequently at an enormous expense during the whole period of the quarantine, and perishable goods are materially injured.' In Scotland also, there is not now even a place where vessels, which do not require to discharge and air their cargoes, can even be allowed to lie quarantine. In a recent instance at Liverpool, a vessel (belonging to this port) arriving from Alexandria, was detained seven weeks waiting for her turn at the Lazaretto, while another, which had sailed from the same port subsequent to her, discharged in twenty-one days, having come in before numerous arrivals, while both were subjected to an exorbitant charge for landing the goods from the Lazaretto. Were the Government even to establish proper depots in England, say at Milford Haven and Liverpool for the West, and Stangate Creek and Hull or Grimsby for the East Coast, (denying still however the necessity of this) where vessels could discharge and load an outward cargo of other goods, the homeward cargo might be sent coastwise at a much less expense than that incurred by the detention of a vessel and crew for six weeks; and it would be conferring a boon on the shipping interest by removing a restriction on commerce which, with reference to Egypt alone, from the present aspect of affairs, may be expected greatly to increase.

IV. If the Quarantine Laws are permitted to exist, proper instructions should be given to Consuls at the different suspected ports, that they might be able to inform masters of vessels whether they are to proceed to Stangate Creek or to the port of their final destination. On one occasion, a vessel arrived at this port from Smyrna, and was ordered back to Stangate Creek, though directed by the Consul to proceed to the port of discharge. Another at Alexandria could obtain no information from the Consul, and is even now compelled to call at Cork or Falmouth, after the result is ascertained here.

The foregoing Report was unanimously approved of, and the former Committee was continued, with instructions forthwith to memorialize the Government on this very important matter, and to communicate with the several Chambers of Commerce throughout the country on the subject.

INDEX.

Aachen, or Aix la Chapelle, on its springs and baths 481
Acid, hydrochloric, its hurtful operation on trees 512
Albuminous urine, case of, in disease of hip-joint 39
Amputation at shoulder joint, case of 353
——— of thigh, for lacerated wound of knee joint 355
Aneurism of cœliac axis 23
Aneurism of heart, cases of 261
Artery, cœliac, case of aneurism of 23
Artery, coronary, case of rupture of 31
Beetles, instance of expulsion of, from stomach 384
Bennett, Dr James Henry, on inflammation of womb 226
Blaps mortisaga, church-yard beetle, see beetle and stomach 384
Blood, on the size of the red corpuscles in birds 239
Braid, Mr James, on the power of the mind over the body 286
Brain, on its minute structure 313
Bread, unfermented, on the preparation of 248
Bread, on the preparation of 500
Brodie, Sir B., his lectures on surgery 232
Calculi of nasal fossæ, history of 251
Cerebro-spinal fluid, on its use 235
Cerebral organ of speech, on 234
China, Dr Wilson's notes on 158
Chusan, See China 158
Clark, Sir James, his work on climate 475
Climate, Sir James Clark on 475
Confinement, on its influence in producing scrofula 238
Cornea, removal of iron sparks from 251
Cox, Mr W. Sands, his memoir on amputation at the hip-joint 195
Eye, case of tumour displacing globe of 6
Fat, on its production 235
Fevers, malarial, Dr Stratton on 74
Fractures, See Thigh-bone 64
Ganglion, on the ophthalmic 84, 312
Geddes, Dr, his illustrations of diseases of India 446
Glasgow, vital statistics of 434
Gums, ulceration of, epidemic 237
Hæmatemesis, case of, from rupture of artery 31
Hall, Dr C. Radcliffe, on the ophthalmic ganglion 84, 312
Health, on the report of commission on that of towns 358
Heart, rupture of its left auricle 18
Heart, on its movements 234
Heart, cases of aneurism of 261, 503
Hernia, case of vagino-labial 254
Hip-joint, case of amputation at 195
Hip-joint, case of diseased, with dropsy 39
Hong-Kong, see China 158
Ileum, case of perforation of 26
Ilio-femoral articulation, amputation at 195
India, statistical illustrations of diseases of 446
Infirmaries, statistics of those of Edinburgh and Glasgow 108
Iodine employed to remove sparks of iron from the cornea 251
Kidneys, disease of 35
Muriatic acid, its effects in injuring trees
Nasal passages, on calculi of 251
Necrosis of maxillary bones produced by vapour of phosphorus 254
Nux vomica, its extract in prolapsus ani 256
Œdema of infants, on its nature 236
Œsophagus, ulceration of, into trachea 24
Peacock, Dr Thomas B. on aneurism of the heart 261
Penitentiary of Milbank, effects of confinement in 238
Pereira, Dr, his case of aneurism of heart 503
Phillip's, Mr Benj. on scrofula 117
Phosphorescence, case of, in human body 285
Phosphorus causes necrosis of maxillary bones 254
Pickells, Dr William, his case of beetles in human stomach 384
Placenta, on cases of its presentation 47
Poisoning, case of, by arsenic 43

Prisons, on confinement in, as a cause of disease . . 238
Lectures on surgery, Sir B. Brodie's 232
Lee, Mr Edwin, on the mineral waters of Germany 481
Lung, case of gangrene of 10
Lung, tubercular disease of 12, 13
Madeira, on the age of puberty in 281
Magnetism, animal, see mind and mesmerism 286
Marshall, Mr H., on the military force of Britain, and military punishments 203
M'Cormack, Dr H., his case of phosphorescence in human body 285
Mesmerism, explanation of its phenomena . . 286
Military punishments, history of 203
Milk of nurse influences nutrition of infant . . 251
Mind, on its power over the body 286
Mortality after great operations, on means of avoiding . 251
Mortality of Edinburgh and Leith, statistics of . . 256
Phthisis, case of . . . 13
Prolapsus uteri and Leucorrhœa, what they indicate 227
Prolapsus of uterus caused by incision of portion of vagina 254
Protuberance annular, tubercle of 236
Puberty, on its period in Hindu women . . 56
———— in Madeira . 281
Punishment, military, history of 203
———————— evidence in a case of, terminating fatally 395
Roberton, Mr John, on puberty in Hindu females . 56
————————on puberty in females in Madeira . 281
Russell, Mr James, on placenta prævia and its treatment 47
Sanatory state of towns, on 358
Scrofula, *Mr Benjamin Phillips* on 117
Scrofula caused by confinement 238
Seminal losses, on the treatment of 250
Speech, on the cerebral organ of . 234
Spermatozoa of the bear, on 233
Springs, mineral, on those of Germany . . 488
Statistics of Edinburgh and Glasgow Infirmaries . 108
Statistics of Glasgow, vital . 434
Stomach, case of perforation of 25
Stomach, case of rupture of 27
Stomach, cases of perforation of 34
Stomach, beetles expelled from human . . 384
Stratton, Dr, on malarial fevers 74
Syphilis, on the question of its communication to the brute animals 248
Thigh-bone, on fractures of its neck 64
Thomson, Mr J. his statistics of Edinburgh and Glasgow Infirmaries . . 108
Thomson, Dr Robert Dundas, on food and the preparation of bread 48
Trees and vegetables destroyed by muriatic acid vapours 512
Tumour displacing the globe of the eye . . . 6
Uterus, on chronic inflammation of 226
Venereal warts, on treatment of 255
Waters, mineral, on those of Germany . . 481
Watt, Dr A., his vital statistics of Glasgow . . 434
Watson, Dr Alex. on fractures of neck of thigh-bone . 64
Wetzlar, Dr L., on the springs of Aix-la-Chapelle . 481
Wilson, Dr, notes by, on China, and diseases of . . 168
Womb, on inflammation of . 226
Young, Dr, his observations on the sanatory measures required in large towns 358

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